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Free and Coerced Transatlantic Migrations: Some Comparisons

DAVID ELTIS

MOST OF THE GREAT PARADOXES thrown up by the development of the Western world have been played out in the population shift westward, to the Americas. The tensions between freedom and order, as well as the material inequalities in the Old World societies that impelled many Europeans to make the move to the Americas, were in some ways exacerbated rather than resolved by the shift. Politically, wars of independence generated new nation-states free from the tyranny of the Old World but in many cases having as their foundation a caste of slaves. Economically, unprecedented income levels, more evenly distributed than ever before, in one part of the Americas were supported in part by an equally unprecedented exploitation of labor in another. Demographically, phenomenal rates of natural increase among many immigrant populations, including slaves, went together with the near annihilation of the indigenous population of both North and South America.

Although the contradictions have fascinated historians and generated widely differing interpretations of the settlement of the Americas, there has been a tendency to write of the two immigrant flows, free and coerced, in isolation from one another. And, until recently, European migration has received the major share of scholarly (as opposed to polemical) attention.¹ The present essay attempts an assessment of the relative magnitude of these flows, particularly in the middle quarters of the nineteenth century, before surveying some of the contributing factors, the conditions of transportation, and some aspects of the postmigration experience of the two groups.

MODERN MIGRATION TO THE AMERICAS has lasted nearly five hundred years. The flow from Europe is essentially complete; the last major wave came in the 1920s. African migration was cut off more abruptly in the 1860s: the first significant reduction came at the end of 1850, when the Brazilian traffic was suppressed. As

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¹ For two works that are exceptions to the rule, see Ralph Davis, *The Rise of the Atlantic Economies* (London, 1973), 125; and Nicolás Sánchez-Albornoz, *The Population of Latin America*, trans. W. A. R. Richardson (Berkeley and Los Angeles, 1974), 74–75.

might be expected, peoples of European descent dominated the population of the Americas by the twentieth century, making up 65 percent of the total in 1935. Those of African descent formed only 14 percent of the population at the same time. But the European majority emerged long before the beginning of this century. A hundred years earlier, in 1835, as the slave trade entered its final years and before the great waves of European migration began, the equivalent percentages were 50 and 24.² Such statistics do help explain the relative scholarly neglect of the African traffic, even though archival source material can only be described as abundant. But twentieth-century population figures, as Philip D. Curtin has suggested, make a treacherous foundation for estimating the scale of immigration flows, and recent work based partly on actual counts and partly on inferences derived from estimates of early populations and vital rates has allowed the generation of more reliable assessments of immigration. As the nineteenth century saw the beginnings of major changes in the relative strengths of the free and slave traffic, it is convenient to divide our discussion of the volume of these flows into the years before and after 1820.

A rough estimate of African arrivals for any major region in the New World and for any period longer than a decade is much easier to make today than it was fifteen years ago. Curtin's benchmark figures for the slave trade have been subject to some minor revisions, especially for the late eighteenth and nineteenth centuries, and they remain the starting point for most calculations of the volume of the traffic. The best recent estimates suggest that by 1820 just over eight million Africans had arrived in the Americas.³ European immigration figures for the same period cannot be derived so easily: neither the shipping records nor demographic estimates are available. Actual white immigration figures may be inferred from recent work on British colonial populations, both aggregates and vital rates, which make possible residual calculations of net arrivals by decade.⁴ Using this procedure, historians have estimated that down to 1820 about 850,000 white immigrants entered the British Caribbean possessions and what became the United States. Applying the same method to French, Dutch, and Danish colonial population data and assuming the same vital rates as those that obtained in the British Caribbean, scholars have calculated another 264,000 immigrants.⁵ Data for the Iberian

² R. R. Kuczynski, *Population Movements* (Oxford, 1936), 102–03; and Angel Rosenblat, *La Población indígena y el mestizaje en America*, 2 (Buenos Aires, 1950): 36. Also see the discussion in C. Vann Woodward, *American Counterpoint: Slavery and Racism in the North-South Dialogue* (Boston, 1964), 79–91.

³ The archival work stimulated by Philip D. Curtin's *The Atlantic Slave Trade: A Census* (Madison, Wisc., 1969) has been reviewed by Paul E. Lovejoy in "The Volume of the Export Trade in Slaves: A Synthesis," *Journal of African History*, 23 (1982): 473–501.

⁴ David W. Galenson, *White Servitude in Colonial America: An Economic Analysis* (New York, 1981), app. H. Also see Henry A. Gemery, "Emigration from the British Isles to the New World, 1630–1700: Inferences from Colonial Populations," *Research in Economic History*, 5 (1980): 179–231.

⁵ Gustav Sundbärg has estimated that 160,000 immigrants came to the United States from 1801 to 1820; Sundbärg, *Aperçu statistiques internationales* (New York, 1908), 108. Since David Galenson's estimates cover the period to 1780, the years 1781–1800 form the only period for which no data exist. War conditions during most of this period, however, probably kept the number of immigrants down. Adam Seybert estimated annual immigration during the 1790s at 6,000 per year; Seybert, *Statistical Annals: Embracing Views of the Population, Commerce, [and] Navigation . . . of the United States of America* (Philadelphia, 1818). For summaries of the French, Dutch, and Danish colonial data, see John James McCusker, "The Rum Trade and the Balance of Payments of the Thirteen Colonies, 1660–1775" (Ph.D. dissertation, University of Pittsburgh, 1975), 600–01, 705–11; and,

American areas, however, are not responsive to even these crude approaches. The white population of Spanish and Portuguese America was much smaller than that in the United States in 1825, but, had the rate of natural increase in the former regions been much lower, they could have received more immigrants. Higher rates of Iberian immigration are, however, improbable for a number of reasons.

Regions of Iberian settlement may conveniently be divided into two broad categories. Brazil and mainland Spanish America outside the tropics and the high-lying regions within the tropics had a climate, epidemiology, and resource base not dissimilar to that of the United States and almost certainly had a positive rate of natural population increase, one that was greater than the increase experienced by the Spanish Caribbean and most of Central America. The white population of the first of these regions, South America and what became Mexico, grew from 115,000 in 1570 to about 600,000 in 1650 and 3,800,000 in 1825.⁶ Although by any standard other than the North American this increase seems impressive, these figures represent a growth rate of only slightly more than 10 percent per decade, compared to rates for the United States of close to 30 percent. Thus, unless the rate of natural increase was very low, immigration could not have been of great significance.⁷

The second region, the low-lying area of Spanish Central America and the Caribbean, seems demographically similar, for the population increased sevenfold between 1650 and 1825. But, if the circumstances of the British West Indian colonies are any guide, these regions may well have faced a high rate of natural decrease with accordingly high population inflows. It seems more probable, however, that by the mid-seventeenth century the surviving descendants of Spanish immigrants had adapted to the severe epidemiological environment, much as the Indian had at about the same time become less susceptible to European disease and that both groups, therefore, subsequently sustained strong rates of natural increase.⁸ The key difference between English and Spanish subtropical America was that the area of Spanish settlement was older; thus, the inhabitants had more time to develop resistance to the disease environment. The decrease in new arrivals was certainly consistent with a more rapid rate of natural increase,⁹ and, in New Spain

for post-1780 data, see Alex Moreau de Jonnés, *Recherches statistiques sur l'esclavage colonial et sur les moyens de le supprimer* (Paris, 1842), 16–21.

⁶ These data are Angel Rosenblat's, with the addition of a guestimate for Peru in 1825 and an adjustment for Dutch, French, and English settlements on mainland South America in all of these years; see Rosenblat, *La Población indígena*, 36–39, 59, 88. Except possibly for data on Mexico, subsequent investigations have not displaced Rosenblat's estimates; see Dauril Alden, "The Population of Brazil in the Late Eighteenth Century: A Preliminary Study," *Hispanic American Historical Review*, 43 (1963): 173–205; Oliver Onody, "Quelques traits caractéristiques de l'évolution historique de la population de Brésil," in Paul Deprez, ed., *Population and Economics*, Proceedings of the Fourth Congress of the International Economic History Association, 1968 (Winnipeg, 1968), 335–64; and Woodrow W. Borah and Sherburne F. Cook, *Essays in Population History*, 3 vols. (Berkeley and Los Angeles, 1971–79).

⁷ A rate of natural increase of 5 percent per decade (or one-sixth the rate in North America), allowing for deaths of 10 percent within the first year of arrival, yields a total immigration of 935,000 over the period 1650–1825. But a rate of natural increase as low as 5 percent seems very unlikely.

⁸ For the timing of the change, see Sanchez-Albornoz, *The Population of Latin America*, 86–112. The Brazilian Indian population, however, has yet to reverse this secular decline, at least in aggregate; John Hemmings, *Red Gold: The Conquest of the Brazilian Indians* (London, 1978), 487–501.

⁹ Philip D. Curtin, "Epidemiology and the Slave Trade," *Political Science Quarterly*, 83 (1968): 213–14.

at least, the most populous of the Spanish New World colonies, fragmentary mission records suggest that the rate of natural increase of the Mexican *vecinos* ("householders") matched those of New France and colonial America.¹⁰

Iberian immigration, then, could have exceeded its British equivalent only if the rate of natural increase of the population of Iberian America had lagged far behind that of British America.¹¹ As this seems unlikely, it appears that Iberian immigration was less than 800,000, the figure for British immigrants. But we can estimate the figure more precisely. Hispanic immigration was closely regulated by the state, and records of actual departures exist. Work on these as well as on shipping records has led to independent estimates of immigration into Spanish America of 437,000 immigrants down to 1650 by one scholar and 750,000 to 1700 by another.¹² Unlike the aggregates for the British Americas, neither of these allow for return flows, which were certainly significant. Eighteenth-century data on Spanish immigration is particularly thin, but the numbers were certainly much lower than in previous centuries.¹³ Perhaps an assumption is in order here that net immigration between 1700 and 1820 was equal to net repatriation prior to 1700. The figure of 750,000 may thus be appropriate for net immigration into Hispanic America down to 1820. If we add to this 500,000 arrivals in Brazil based on Magalhaes-Godino's estimates of Portuguese emigration,¹⁴ we arrive at a pre-1820 aggregate for Iberian America of about one and a quarter million. Such a total means annual immigration of only a few thousand, but even this low figure appears high in light of the population trends of Spanish America. But, although a plausible set of vital rates consistent with such an estimate is difficult to construct, that is not really necessary here. Even if we accept a high figure for Iberian immigration, the total net migration from

¹⁰ Borah and Cook, *Essays in Population History*, 3: 311.

¹¹ If Iberian America had the same level of immigration between 1570 and 1825 that British America did, and, if 10 percent of the immigrants died during their first year, then Rosenblat's population estimates yield a rate of natural increase of only 7 percent per decade during these two hundred and fifty years.

¹² Peter Boyd-Bowman, *Patterns of Spanish Emigration to the New World (1493–1580)* (Buffalo, N.Y., 1973). For shipping data, see Huguette Chaunu and Pierre Chaunu, *Séville et l'Atlantique (1504–1650)*, 8 vols. (Paris, 1955–59); and, for estimates based on these data, see Magnus Mörner, "Spanish Migration to the New World prior to 1810: A Report on the State of Research," in Fredi Chiapelli *et al.*, eds., *First Images of America: The Impact of the New World on the Old* (Berkeley and Los Angeles, 1976), 767; and Woodrow Borah, "The Mixing of Populations," *ibid.*, 708.

¹³ M. Hernandez y Sanchez Barba estimated Spanish transatlantic emigration at 53,000 for the eighteenth century; Hernandez y Sanchez Barba, "La Población hispanoamericana y su distribución racial en siglo XVIII," *Revista de Estudios Políticos*, 78 (1954): 117–18. Relatively fertile and underpopulated Santo Domingo received only 2,500 immigrants between 1720 and 1764; Marlin D. Clausner, *Rural Santo Domingo: Settled, Unsettled, and Resettled* (Philadelphia, 1953), 35.

¹⁴ Vitorino Magalhaes-Godinho has estimated 1 to 1.5 million departures from Portugal to all destinations between 1500 and 1760, with 0.5 to 0.6 million for the years 1700–60, and he noted that Brazil became of major importance only in the eighteenth century; Magalhaes-Godinho, "L'Émigration portugaise du XV^e siècle à nos jours: Histoire d'une constante structurale," in *Conjoncture économique—structures sociales: Hommage à Ernest Labrousse*, École Pratique des Hautes Études, VI^e Section, no. 47 (Paris, 1974), 254–55. If we assume that emigrants to regions other than the Americas after 1700 were approximately equal to emigrants to Brazil before 1700, then Magalhaes-Godinho's estimate of 0.5 to 0.6 million to all regions for the years 1700–60 may be accepted for emigration to Brazil for the whole period down to 1760. These estimates are exclusive of mortality in transit and return flows, which means that net immigration was perhaps two-thirds of this figure. For the period 1760–1820, the collapse of the mining boom and temporary economic revival in Portugal ensured very low levels of immigration. A net Brazilian immigration figure of 500,000 for the years down to 1820 thus appears possible. Celso Furtado has estimated Portuguese emigration to Brazil at 300,000 to 500,000 during the eighteenth century; Furtado, *The Economic Growth of Brazil*, trans. Ricardo W. de Aguiar and Eric Charles Drysdale (Berkeley and Los Angeles, 1971), 81.

Europe to the Americas before 1820 cannot have been much above two million. Therefore, for every European who came to the Americas before the massive population shifts of the nineteenth and twentieth centuries got underway, at least four and perhaps five Africans disembarked.

Yet not just European immigration accelerated after 1820. Industrialization in Europe ensured an expanding demand for plantation produce and the labor with which it was produced. Industrialization also generated attitudes inimical to both slavery and the slave trade, and a byproduct of the abolition campaigns was the collection of statistics on the slave trade by the British Foreign Office. These data make possible estimates of annual African arrivals in the Americas for the last half-century of the trade. Comparing African arrivals and European immigrants—those Europeans migrating to Brazil, Canada, and the United States, which together received virtually all of the free transatlantic migrants prior to the 1880s—is instructive. Since the available figures make no allowance for repatriation, which for Africans was negligible but which for Europeans may have ranged from 10 to 20 percent of total immigration,¹⁵ the comparison is even more striking. Four times as many Africans as Europeans arrived in the Americas in the 1820s, and, allowing for European repatriation, the two flows were of approximately the same strength in the 1830s. Not until 1840 did arrivals from Europe permanently surpass those from Africa. (See Table 1.) Indeed, in every year from about the mid-sixteenth century to 1831, more Africans than Europeans quite likely came to the Americas, and not until the second wave of mass migration began in the 1880s did the sum of net European immigration start to match and then exceed the cumulative influx from Africa. Slavery and the slave trade did not survive industrialization by more than a generation or two, but the initial impact of the latter was to send more Africans than Europeans to the Americas in the first half of the nineteenth century. But for the abolition of the trade by the major Western powers, moreover, the flow from Africa would undoubtedly have been even larger than it was.

IN TERMS OF IMMIGRATION ALONE, THEN, America was an extension of Africa rather than Europe until late in the nineteenth century. Obviously, the demographic trends established in the aftermath of immigration had ensured that the Americas would be predominantly white long before the avalanche of European arrivals in the half-century before 1930. But before examining the vital rates some commentary on the aggregates is necessary.¹⁶ The basic conditions that impelled and

¹⁵ On the repatriation of migrants, see J. D. Gould, "European Inter-Continental Migration—The Road Home: Return Migration from the U.S.A.," *Journal of European Economic History*, 9 (1980): 41–111; and M. Simon, "The U.S. Balance of Payments, 1861–1900," in *Trends in the American Economy in the Nineteenth Century*, National Bureau of Economic Research, *Studies in Income and Wealth*, vol. 24 (Princeton, 1960), 664–66, 690. There was also a small return flow of Africans to what became Liberia as well as from Bahia to the Bight of Benin after the slave revolts of the 1830s and from Rio de Janeiro to the Congo in the 1850s. But these were probably more than offset by the flow of Africans to the British and French Caribbean in the 1840s and 1850s in government-supervised migration schemes.

¹⁶ For our purposes, the traditional categorization of immigrants by source—namely, northwest and southeast Europe—is replaced by a breakdown by occupation. One group of emigrants were the displaced agricultural workers or artisans, usually without a skill and originating initially in Britain and then in Ireland,

TABLE 1
Transatlantic Immigration (in Thousands), 1820–1859

<i>Year</i>	<i>Slaves</i>	<i>Free Migrants</i>	<i>Year</i>	<i>Slaves</i>	<i>Free Migrants</i>
1820	50.0	10.3	1840	47.0	102.6
1821	57.1	6.9	1841	33.2	104.6
1822	59.0	5.4	1842	25.3	157.0
1823	35.0	5.0	1843	42.4	89.8
1824	57.6	5.9	1844	36.3	98.5
1825	64.9	9.5	1845	23.6	141.9
1826	58.0	10.7	1846	50.8	189.8
1827	57.8	32.4	1847	61.3	322.9
1828	73.5	38.6	1848	61.6	250.2
1829	88.9	29.1	1849	63.6	328.0
1820–29	602.5	153.8	1840–49	445.1	1,785.3
1830	64.4	35.2	1850	34.7	286.2
1831	18.3	63.2	1851	9.9	415.1
1832	26.5	79.1	1852	8.1	406.0
1833	30.2	50.8	1853	12.5	413.1
1834	42.4	88.4	1854	11.6	471.5
1835	71.6	54.5	1855	6.7	221.4
1836	73.5	98.3	1856	7.9	220.9
1837	80.3	92.2	1857	12.0	261.0
1838	79.0	37.3	1858	17.3	136.8
1839	77.2	71.7	1859	30.5	132.9
1830–39	563.4	670.7	1850–59	151.2	2,964.9

NOTE: The category of “Free Migrants” includes all arrivals to Canada, the United States, and Brazil. SOURCES: David Eltis, “The Direction and Fluctuation of the Transatlantic Slave Trade, 1821–1843: A Revision of the 1845 Parliamentary Paper,” in Henry A. Gemery and Jan S. Hogendorn, eds., *The Uncommon Market: Essays in the Economic History of the Atlantic Slave Trade* (New York, 1979), 273–98, and “The Direction and Fluctuation of the Transatlantic Slave Trade, 1844–1867,” paper presented at the annual meeting of the African Studies Association, 1981; and Walter F. Wilcox and Imre Ferenczi, *International Migration*, 1 (New York, 1929): 360, 369, 380, 549–50.

southern Germany, and southern Italy. The other group were workers from maturing industrial economies, frequently skilled and searching for a wider range of job opportunities. As might be expected, the latter—at least in the early stages of European migration—came from northwest Europe. The discussion here focuses on the first of these two, since the migration of displaced agricultural workers predominated overall while the slave trade remained open, although skilled workers came to dominate the flow of indentured workers from Britain during the eighteenth century. See Galenson, *White Servitude in Colonial America*, 34–64.

attracted fifty million Europeans to the New World after 1820 had probably existed in some regions for at least two centuries before the shift occurred. Recent work on colonial wealth and on the nutritional implications of trends in human stature suggest that the income differential between Europe and America for the nineteenth and twentieth centuries probably existed in earlier centuries as well. Similarly, the revolutionary agricultural developments that displaced so many Europeans in the nineteenth century had already begun, in England at least, in the late seventeenth century.

But to interpret the move to the Americas purely in terms of differentials in measured income is an unjustified simplification. Transatlantic migration spanned the period in Western Europe when attitudes toward labor and work were undergoing fundamental change. Ultimately, the explanation of migration has to be shaped by our assessment of the goals of preindustrial man and how these changed in the course of industrialization. Wage earners in its first stage often aspired to nonincome goals, such as increased leisure or a degree of independence, which could only be supported by ownership of some land. Indeed, the Chartist land bank program of the 1840s indicates the persistence of this goal beyond the advent of industrialization.¹⁷ Faced with choice, the early- or pre-industrial worker often sacrificed higher income to achieve these goals and generated the backward-bending supply curve for labor.¹⁸ During or immediately preceding industrialization, a significant portion of the labor force has typically broken this pattern and increased both the demand for consumer goods and the quantity of labor supplied.¹⁹ But, for those reluctant or unable to make the adjustment, the Americas provided an apparently certain route to the achievement of these nonincome goals. Thus, for many years it was not the worker of the mature industrial state but rather the victim of economic development who became the vanguard of American immigration. This phenomenon in part explains the gradual but accelerating shift in the focal point of European emigration from the northern to the southern Continent from the seventeenth to the twentieth centuries.²⁰

But the Americas turned out to be somewhat more than merely a haven for preindustrial attitudes. The resource base of much of the New World, the impact of European technology, and the ready availability of export markets for American produce ensured that many American immigrants in both agricultural and nonagricultural occupations could indeed attain an income level high by European

¹⁷ Christopher Hill, "Pottage for Freeborn Englishmen: Attitudes to Wage Labour in the Sixteenth and Seventeenth Centuries," in C. H. Feinstein, ed., *Socialism, Capitalism, and Economic Growth: Essays Presented to Maurice Dobb* (Cambridge, 1967), 338–50; and Eric Foner, *Free Soil, Free Labor, Free Men* (New York, 1970).

¹⁸ The most stimulating general discussion of this phenomenon is Marshall Sahlins's *Stone Age Economics* (New York, 1972), 1–39. For the behavior of the Central American Indians in this context and the Spanish response, however, see William L. Sherman, *Forced Native Labor in Sixteenth-Century Central America* (Lincoln, Neb., 1979), esp. 191–98. For early industrial attitudes in England, see E. P. Thompson, "Time, Work-Discipline, and Industrial Capitalism," *Past & Present*, no. 38 (1971): 56–97.

¹⁹ For a full and lucid discussion of this issue, see Stanley L. Engerman, "Coerced and Free Labor: Property Rights and the Development of the Labor Force," *Annales: Économies, sociétés, civilisation* (forthcoming).

²⁰ The importance of European population growth on migration to the Americas should be recognized here, but, as this was an integral part of the process of industrialization, I have not given it separate treatment in the text.

standards without abandoning the drive for independence and other nonpecuniary goals. Possession of even a small amount of land or the rudiments of a skill in much of North and South America was for many the major route to this dual achievement.

Such a favorable conjunction of circumstances makes the estimates of pre-1820 migration seem small. As an explanation some might point to official hostility to emigration in pre-nineteenth-century Europe. Governments that were convinced of the existence of a backward-bending supply curve for labor to the point of enforcing draconian vagrancy laws and were preoccupied with supplying export markets, for which cheap labor was essential, could hardly be expected to have encouraged an outlet to land-abundant America. Yet England, which saw the triumph of the balance-of-trade theory after 1696,²¹ also sent by far the greatest number of migrants, absolutely and relatively, across the Atlantic. The cost of transportation and lack of knowledge probably provided a more effective barrier. In 1650, at a time when the wage rate for agricultural laborers in southern England was a shilling a day, the fare from England to America was £6—or about five months wages. A well-organized market for indentured labor—a system by which the immigrant received transportation, upkeep, and freedom dues in exchange usually for four years of bound servitude—was one response. But such a route was open only to the young and single, and it gave the servant little control over employer or type of work. Although a second wave of indentured migration—this time from Asia—got underway after 1850, the European masses eventually moved without recourse to the indenture system.

In the course of two hundred years, shipping rates declined steadily, and the average crossing time fell. By the 1850s steerage passengers were paying less than £3, while money wages in English agriculture had tripled. The same process improved the flow of knowledge through mail, newspapers, and word-of-mouth from returnees. As J. D. Gould has pointed out, the prospective immigrant needed firm evidence of conditions in the form of “feedback” from members of his community who had already made the move to the New World before risking the crossing himself.²²

The goals of the migrant naturally influenced the direction of the migrant flow. Even in the sixteenth century most emigrants were hired retainers rather than bureaucrats, soldiers, or merchants.²³ They were without land, and they left societies in which land was the basis of power and wealth. While large Indian populations and the *encomienda* and *repartimiento* systems restricted opportunities—relative to Europe—for the early Spanish American immigrant, all of the Americas were land abundant. They were also capable of producing a wide variety of

²¹ Joyce Oldham Appleby, *Economic Thought and Ideology in Seventeenth-Century England* (Princeton, 1978), 248–79; and Klaus E. Knorr, *British Colonial Theories, 1570–1850* (Toronto, 1944), 68–81.

²² J. D. Gould, “European Inter-Continental Emigration: The Role of ‘Diffusion’ and ‘Feedback,’” *Journal of European Economic History*, 9 (1980): 267–315. Also see Marianne Wokeck, “The Flow and Composition of German Immigration to Philadelphia, 1727–1777,” *Pennsylvania Magazine of History and Biography*, 105 (1981): 276–78.

²³ Peter Boyd-Bowman, “Patterns of Spanish Emigration to the Indies until 1600,” *Hispanic American Historical Review*, 56 (1976): 584.

products with widely differing production functions and climate requirements. Grain, livestock, and the early transatlantic staples such as fish, fur, and tobacco could be produced in the temperate zones of the Americas without any considerable economies of scale. Such items could in most cases be produced on a family farm or its hunting or fishing equivalent, and such an enterprise was therefore entirely consistent with the aspirations of the European immigrants. Other major American commodities, such as sugar, coffee, and cotton, however, were most efficiently produced on large plantations where gang labor and high labor intensities could be utilized.²⁴ The small farmer could not compete with such an operation, nor was he prepared to join its labor force unless the compensation was at a level that would have made the plantation unprofitable. A land-hungry, job-seeking immigrant would thus have tended to avoid areas with a high incidence of plantations.

The potential migrant needed to be able to compare the present value of future net expected returns of both a pecuniary and a nonpecuniary nature in the country in which he lived on the one hand with those of the countries of possible immigration on the other. The pecuniary benefits of moving to the Americas were significant perhaps from the time of the first settlement, but most potential immigrants had little chance of judging nonpecuniary advantages before the mid-nineteenth century; in other words, transportation costs and lack of knowledge limited the immigrant flow. Letters home from immigrants are now readily available for several different language groups spanning three hundred and fifty years.²⁵ These suggest that the pattern of settlement both before and probably after the mid-nineteenth century was heavily influenced by nonpecuniary factors, such as the desire for independence (often through owning land), the desire to avoid plantation-type working conditions or the high "seasoning" mortality of the tropics, or the desire to find a congenial locale in terms of language and climate. And the prospective employer in the Americas who wished to hire labor where conditions inimical to these aspirations existed had to pay a premium, and indeed a good part

²⁴ All participants in the debate on the relative efficiency of Southern agriculture would agree that employing slaves was more profitable in the South than in the North. Extra labor input from the slave, as opposed to the free worker, was available to both the Northern farmer and the Southern planter, but only the planter chose to use slaves. That suggests that the plantation was able to derive an extra advantage from employing slaves that was not open to the Northern farm. The farmer accordingly could not match the price the planter was prepared to pay. For a restatement of the argument that a gang labor system explains the superior efficiency of Southern agriculture, see Robert W. Fogel and Stanley L. Engerman, "Explaining the Relative Efficiency of Slave Agriculture in the Antebellum South: Reply," *American Economic Review*, 70 (1980): 672–90. This "reply" is a response to Paul A. David and Peter Temin's "Explaining the Relative Efficiency of Slave Agriculture in the Antebellum South: Comment," *ibid.*, 69 (1979): 213–18, and Gavin Wright's "The Efficiency of Slavery: Another Interpretation," *ibid.*, 219–26.

²⁵ "Letters from Labouring Emigrants," in *Emigration in the Victorian Age: Debates on the Issue from Nineteenth-Century Critical Journals* (Farnborough, Hants., 1973); Theodore C. Blegen, ed., *Land of Their Choice: The Immigrants Write Home* (Minneapolis, 1955); and Charlotte Erickson, *Invisible Immigrants: The Adaptation of English and Scottish Immigrants to Nineteenth-Century America* (London, 1972), 22–31, 81–226, 233–40, 265–389. James Lockhart and Enrique Otte, eds., *Letters and People of the Spanish Indies: Sixteenth Century* (London, 1976), is also useful, but the selection of letters Lockhart and Otte published does not reflect the occupational distribution of the sixteenth-century Spanish migrants: merchants and officials are overrepresented. For new evidence of German migration consistent with the present discussion, see Marianne Wokeck, "The Flow and Composition of German Immigration to Philadelphia, 1727–1775," *Pennsylvania Magazine of History and Biography*, 105 (1981): 249–78, esp. 274–78.

of the difference between the costs of free and slave labor in the Americas was probably made up of this compensatory premium that the slaveowner did not have to pay.

Under slavery the decision on transatlantic movement was, of course, shifted from the worker to the owner, though the transfer usually implied that an additional factor—control of the slave—had to be figured into the cost of operation. The owner, however, needed to make the same implicit calculation of expected returns, pecuniary and nonpecuniary. Conspicuous consumption has been amply documented as a motive for slave purchases in the Americas, but motives other than material gain were probably more important to the African seller than to the American buyer. Increased supplies of slaves often occurred as a byproduct of African tribal warfare. Reduced supplies, however, could often result from the prestige associated with large kin groups, of which slaves formed a fringe element. In both cases, slaves might be supplied (or withheld) without any change in the market price.²⁶ Yet evidence of a direct relationship between price and quantity of slaves supplied over a long period of time seems fairly strong,²⁷ and pecuniary factors, therefore, were probably rather more important to the slave trade than to the flow of free immigration.

The employment of slaves on plantations in the Americas hinged on the price of the slave—or the present value of the slave's future net earnings—equalling the African cost of purchase plus the transatlantic transportation and American distribution costs. The slaveowner, unlike the employer of indentured labor, could expect to receive a stream of income extending over the lifetime of the slave. If the marginal productivity of the alternative types of labor was equal, then the decision on which type of labor to use would depend first on relative transportation costs from Europe and Africa to the Americas, second on the length of the indenture relative to the life expectancy of the laborer (free and slave), and third on wage levels in America and Europe on the one hand and slave prices in Africa and America on the other. Relative wage levels would reflect the nonpecuniary factors that nonslave labor could take into account, while relative slave prices would reflect the competing uses for slaves in Africa and the Americas.

During the seventeenth century rising wages in England and better opportunities in nonplantation British America increased the price of indentured labor for plantation owners. Plantation slavery, which had appeared first in northern Brazil (the part of America closest to Africa), spread to the eastern Caribbean and eventually—helped by a long decline in sugar prices down to the 1680s—to the North American mainland, although for various political reasons major plantation areas in between, such as St. Domingue and Cuba, were not developed until later.²⁸

²⁶ Philip D. Curtin, *Economic Change in Pre-Colonial Africa*, 1 (Madison, Wisc., 1975): 156–68; and Igor Kopytoff and Suzanne Miers, "African 'Slavery' as an Institution of Marginality," in Kopytoff and Miers, eds., *Slavery in Africa* (Madison, Wisc., 1977), 3–78.

²⁷ E. Phillip LeVeen, *British Slave Trade Suppression Policies, 1821–1865* (New York, 1977), 123–63; and David Eltis, "The Transatlantic Slave Trade, 1821–1843" (Ph.D. dissertation, University of Rochester, 1979), 249–53.

²⁸ Of the extensive literature on the transition from indentured to slave labor in the British Americas, see in particular Richard S. Dunn, *Sugar and Slaves: The Rise of the Planter Class in the English West Indies, 1624–1713* (New York, 1973), 68–74; and Russell R. Menard, "From Servants to Slaves: The Transformation of the

By the nineteenth century supplies of indentured labor had dried up, but the free labor that replaced slavery did nothing to fill plantation needs. European and Asian workers were eventually recruited for the plantations of Brazil, Cuba, and elsewhere, but only Asians came in large numbers before the end of slavery.²⁹

In fluctuations over time as well as in direction, the two population shifts were apparently separate and self-contained. The interaction between transatlantic capital and labor flows and cycles of economic activity in Britain and America is the subject of a long literature. While no consensus has emerged on the economic variables with the most explanatory power, there can be no doubt that the broad surges of free migration seen in Table 1 reflect periods of prosperity and depression in Europe and the Americas and that the pull effect of American prosperity was a particularly important short-run influence.³⁰ It is the figures on slave migration that are at odds with cyclical fluctuations, and this is particularly puzzling in view not only of the close ties between the demand for plantation produce and the demand for slaves but also of the greater homogeneity of the market for slaves than of that for free labor.

The three major plantation crops were cotton, sugar, and coffee, and, although only one of these was an input for manufactured goods, all had high income elasticities in the early nineteenth century. By this time the United States had established its domination of world cotton supply, and, since the slave trade to the United States was effectively suppressed soon after legal abolition in that country, only the sugar and coffee planters outside of the United States had access to an African labor supply. The three major branches of the transatlantic slave trade after 1820 went to the sugar plantations of Bahia, in northern Brazil, and Cuba and to the coffee-producing regions of southern Brazil. Only for Cuba and southern Brazil are the data good enough to sustain evaluation. In both cases, fluctuations in the volume of trade as well as the price of slaves can be explained by the exports of plantation produce on the demand side of the market and by various efforts of the importing nations to suppress the trade on the supply side.³¹ Exports of plantation

Chesapeake Labor System," *Southern Studies*, 16 (1977): 355–90. For a thoughtful survey of that literature and a fresh analysis that incorporates new empirical source materials, see Galenson, *White Servitude in Colonial America*, 141–68.

²⁹ The high slave prices in Cuba, Brazil, and the United States after 1850 seem to suggest that the premium necessary to induce free labor to work on the plantation must have been considerable. For an assessment of the indentured labor flow to the Americas both before and after the abolition of slavery, see Stanley L. Engerman, "Servants to Slaves to Servants: Contract Labor and European Expansion," paper presented to a workshop at the Centre for European Expansion, University of Leiden, 1982.

³⁰ See J. D. Gould's review of the literature in "European Inter-Continental Migration: Patterns and Causes," *Journal of European Economic History*, 8 (1979): 622–70. The best-known attempt to relate migration to cyclical activity remains Brinley Thomas, *Migration and Economic Growth: A Study of Great Britain and the Atlantic Economy* (London, 1973), 83–138.

³¹ Although some slaves were moved from coffee to sugar plantations within Cuba and from northeastern Brazil to Rio de Janeiro, the major geographic shifts of the domestic labor forces, particularly in Brazil, did not occur until after the slave trade closed; see Robert W. Slenes, "The Demography and Economics of Brazilian Slavery, 1850–1888" (Ph.D. dissertation, Stanford University, 1976), 120–78; and Rebecca Scott, "Slave Emancipation and the Transition to Free Labor in Cuba, 1868–1895" (Ph.D. dissertation, Princeton University, 1981). In addition, shipments of slaves from Brazil to Cuba were insignificant and from Cuba to Brazil nonexistent. As long as the slave trade continued, then, there should have been a close relationship between the price and output trends of plantation produce and the imports of slaves from Africa.

produce were broadly responsive to the trade cycles of Britain and America, but efforts to suppress the trade, a largely political factor, were not. And the abolition movement was mainly responsible for the patterns of the slave trade seen in Table 1. Indeed, the importance of efforts to suppress the trade is reflected in changes in the costs of trading in slaves. As long as slavery lasted anywhere in the Americas, prices of slaves exceeded the cost of buying Africans and, under normal conditions, carrying them across the Atlantic. In the 1850s, when the British government paid less than \$25 to transport migrant Africans to Trinidad under Passenger Act regulations, slaves could be purchased on the African coast for \$50 and sold in Cuba for \$1,000 and in the United States for \$1,500.³²

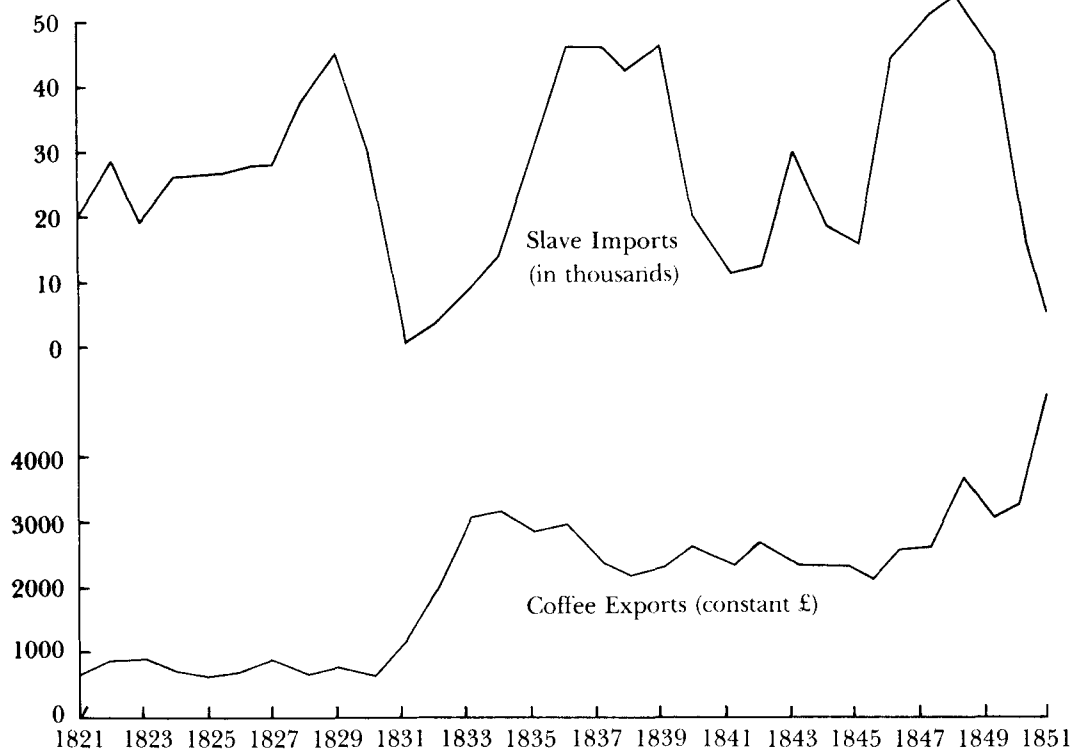
Graphs 1 and 2 show slave imports and real values of exports of the major plantation crops for the two main slave importing regions in the Americas. Although technological improvements, which increased the productivity of the slave, and manumissions, which reduced slave numbers, certainly affected the situation,³³ the produce exports series should serve as a proxy for the demand for slaves. But in neither of the two cases is there a strong relationship between the two series. If abolition is factored in, however, explanation becomes easier. In southern Brazil, the trade for all practical purposes remained open until 1830, when—as a result of a treaty with Great Britain—Brazil declared the traffic piracy and allowed Brazilian slave ships found in international waters to be tried before courts of mixed commission operated jointly by the two countries, courts from which there was no appeal.³⁴ The treaty was signed in 1826, and it stipulated that traffic was to be allowed to continue for three years after ratification, which occurred in 1827. The strong increase in the volume of imports from 1826 to 1830 in the face of coffee exports that in real terms remained static and coffee prices that declined steadily can be explained, it seems clear, only in terms of planters stocking up on slave labor while they still could. No sooner had the trade become illegal than world coffee prices rapidly doubled (between 1830 and 1833) and then remained high until the 1840s. Brazilian exports in real terms quadrupled between 1830 and 1836.

³² Johnson U. J. Asiegbu, *Slavery and the Politics of Liberation, 1787–1861* (New York, 1969), 144. For prices of African slaves in Cuba and Africa in the last years of the slave trade, see Joseph T. Crawford to Lord John Russell, December 16, 1859, Public Record Office, London, Foreign Office (Slave Trade Series) 84 [hereafter, FO 84], volume 1073; Joseph T. Crawford to Lord John Russell, February 5, and September 30, 1861, FO 84, 1135; Edmund Gabriel to the Earl of Malmesbury, April 4, 1859, FO 84, 1075; and Manuel Moreno Fraginals *et al.*, “The Level and Structure of Slave Prices on Cuban Plantations in the Middle of the Nineteenth Century: Some Comparative Perspectives” (unpublished paper, 1982). Cargoes in poor condition, of course, often sold for much less in Cuba, and I have made no attempt here to derive a weighted average. In addition, the U.S. price cited here is for slaves born in the United States; U.S. prices were still not high enough to induce significant imports into the United States. See David Eltis, “The Direction and Fluctuation of the Transatlantic Slave Trade, 1821–1843: A Revision of the 1845 Parliamentary Paper,” in Henry A. Gemery and Jan S. Hogendorn, eds., *The Uncommon Market: Essays in the Economic History of the Atlantic Slave Trade* (New York, 1979), 289, and “The Direction and Fluctuation of the Transatlantic Slave Trade, 1844–1867,” paper presented at the annual meeting of the African Studies Association, 1981.

³³ Changes in productivity probably influenced the demand for slaves far more than manumissions did. Robert L. Paquette has estimated manumissions at only a few hundred in the 1850s; Paquette, “The Conspiracy of La Escalera: Colonial Society and Politics in Cuba” (Ph.D. dissertation, University of Rochester, 1982). The effect of both was to increase the demand for imported slaves.

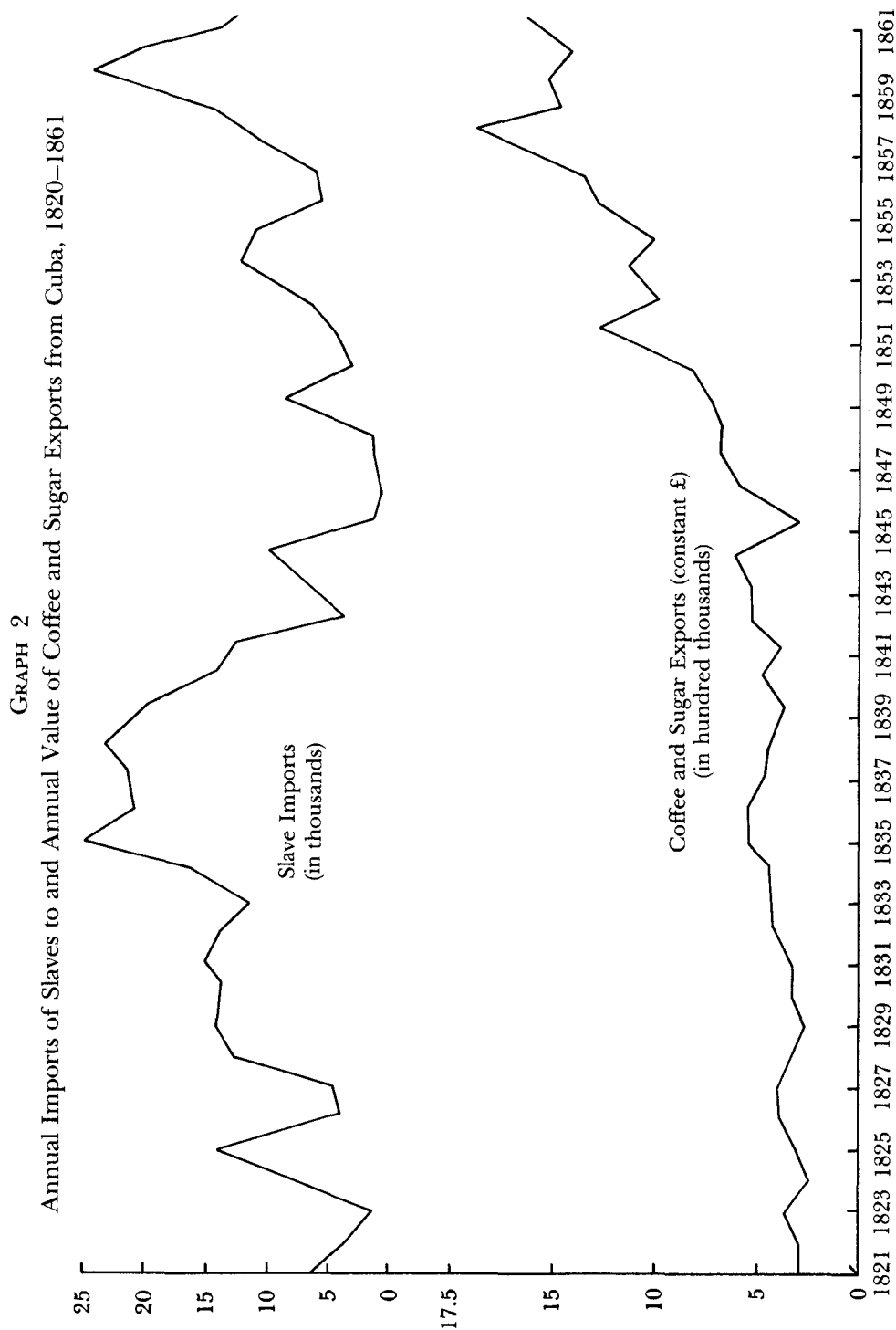
³⁴ L. M. Bethell, “The Mixed Commission for the Suppression of the Transatlantic Slave Trade,” *Journal of African History*, 7 (1966): 79–93.

GRAPH 1
Annual Slave Imports into Southern Brazil and
Annual Value of Coffee Exports from Rio de Janeiro, 1821–51



SOURCES: David Eltis, "The Direction and Fluctuation of the Transatlantic Slave Trade, 1821–1843: A Revision of the 1845 Parliamentary Paper," in Henry A. Gemery and Jan S. Hogendorn, eds., *The Uncommon Market: Essays in the Economic History of the Atlantic Slave Trade* (New York, 1979), 273–98, and "The Direction and Fluctuation of the Transatlantic Slave Trade, 1844–1867," paper presented at the annual meeting of the African Studies Association, 1981; and Instituto Brasileiro de Geografia e Estatístico, *Anuario Estatístico do Brasil, Ano V: 1939–1940* (Rio de Janeiro, 1940), 1374.

The response of the slave trader was at first muted: piracy was, after all, a capital offense, and, even without penalties to the person, a slaving expedition represented a substantial investment. In fact, as the upsurge in the volume of the trade in the early 1830s indicates (see Graph 1), the possibility of loss of any kind was rapidly perceived to be small. No one was convicted in a Brazilian court for importing slaves before 1850, and the courts of mixed commission and the British navy, which provided them with business, could do no more than confiscate slave ships. Even here, however, the ingenuity of the slave trader ensured that losses would be small. Despite an extensive diplomatic campaign, a fleet of cruisers, which eventually numbered over thirty ships, and an unprecedented piece of legislation that allowed British ships to capture and destroy Brazilian and Portuguese slave ships without the permission of the governments of Brazil and Portugal, the main impact the British made on the trade came early in the 1840s, when the ratio of losses to all



NOTE: Sugar prices used in this series are for Havana sugar (ordinary yellow) from the *London Mercantile Price Current* exclusive of British duty. Coffee prices are for Brazilian coffee in Rio de Janeiro. Both price series have been converted to real terms using the indices in Gayer *et al.*, *The Growth and Fluctuation of the British Economy, 1790–1850*, 1 (New York, 1975): 468, and in Paul Rousseaux, *Les Mouvements de fond de l'économie anglaise, 1800–1913* (Louvain, 1938), 266. No adjustment has been made for shipping costs, which are included in the London price. The series is not an FOB export series, although the trends over time of such a series would be almost identical to the data shown here.

SOURCES: Eltis, "Transatlantic Slave Trade, 1821–1843," and "Transatlantic Slave Trade, 1844–1867"; Great Britain, *Parliamentary Papers, 1856*, 55: 589, and 1863, 67: 299; Manuel Moreno Fraginals, *El Ingenio, complejo económico social cubano del azúcar*, 3 (Havana, 1978): 35–36; Cuban Economic Research Project, *A Study of Cuba: The Colonial and Republican Period* (Coral Gables, Fla., 1965), 75; and Instituto Brasileiro de Geografia e Estatística, *Anuário Estatístico do Brasil, Ano V: 1939–40*, 1374.

voyages attempted rose to 39 percent and the volume of imports dipped. Both phenomena were temporary.³⁵ The trade did not end until the British navy began to take action within Brazilian territorial waters in 1850, and in response the Brazilian government began to enforce suppression on its own.³⁶

In Cuba fluctuations in the volume of slaves may be explained in a rather similar fashion. Until the mid-1840s coffee and sugar were both important exports, although sugar grew most rapidly and by the late 1840s was already making up over 75 percent of total exports. Once more, the plantation produce exports series charts the steady increase in the demand for slaves, but the major fluctuations appear to be a response to actual or anticipated attempts to suppress the traffic. The Cuban equivalents of the 1826 Anglo-Brazilian treaty were the Anglo-Spanish conventions of 1814 and 1817, which provided for abolition in 1820. The resulting uncertain slave-trading climate as well as two very successful years for the British cruisers in 1822 and 1823 do much to explain the rise and fall of Cuban slave exports between 1815 and 1823. The downward trend, however, also coincides with falling sugar and coffee prices. The major decline in the early 1840s also coincides with declining sugar prices, but this time the decline in slave imports continued, with only a short break, until 1857 in the face of a rapid recovery of Cuban sugar production. The labor input for this expansion came not from Africa but from rapidly declining coffee production and, to a lesser extent, from the inflow of Chinese contract labor.³⁷ Contributing to these events was the first effective enforcement of abolition by the Spanish colonial authorities. The efforts of Captain General Valdes were followed by the proclamation of 1845, which greatly

³⁵ David Eltis, "The Impact of Abolition on the Atlantic Slave Trade," in David Eltis and James Walvin, eds., *The Abolition of the Atlantic Slave Trade* (Madison, Wisc., 1981), 170–73.

³⁶ Trade in slave prices are consistent with these events. In real terms, prices quadrupled between 1825 and 1834 as the growth of demand outstripped the increase in supply, declined about 60 percent in the next six years as slave traders re-entered the business after formal abolition, and tripled again when abolition reduced supply once more in 1850. See Robert Hesketh to Lord Palmerston, March 10, 1851, enclosing "Paper Showing the Price of Slaves at Rio de Janeiro . . ." FO 84, 848. Also see Stanley J. Stein, *Vassouras: A Brazilian Coffee Country* (Cambridge, Mass., 1957), 229. A regression analysis for fifteen of the years between 1825 and 1850 for which data are available supports this interpretation. Coffee exports (values) and slave prices, both in real terms, explain only 34 percent of the variations in slave imports in these years. The addition of a variable that reflects the activities of the British antislavery squadron—namely, the ratio of captured slave ships to total slave expeditions—raises the percentage of variation explained to 75 ($r^2 = 0.75$). With standard errors in the first set of parentheses and the F statistic in the second set, the equation is

$$M = 34,242 - 456.9S + 16.3C - 116,406L$$

(94.2)	(3.7)	(27,819)
(23.2)	(19.5)	(17.5)

where M = Slaves imported into Brazil south of Bahia,
 S = Price of slaves (in £s sterling)
 C = Value of Brazilian coffee exports (in £s sterling), and
 L = Loss ratio.

For the sources of the data used, see the notes to Graphs 1 and 2, pages 263–64. In the Cuban case, fluctuations in the value of sugar and coffee exports over the forty-one years shown in Graph 2 account for only 23 percent of the annual changes of slave imports. The absence of good price data has, however, prevented further analysis along the lines of the Brazilian case.

³⁷ Francisco Perez de la Riva y Pons, *El Café: Historia de su cultivo y explotación en Cuba* (Havana, 1944), 74–76. For the transfer of slaves into sugar production, see J. Kennedy and W. Dalrymple to Aberdeen, May 9, 1845, FO 84, 561, and *ibid.*, January 1, 1846, FO 84, 620. For an annual breakdown of Chinese arrivals in Cuba, see Hugh Thomas, *Cuba or the Pursuit of Freedom* (London, 1971), 1571.

increased the severity of the penalties for slave trading.³⁸ The 1845 law was widely expected to bring the Cuban slave trade to an end.

The trend in Cuban slave prices is consistent with this interpretation. Prices declined 40 to 50 percent from a postabolition high of \$550 during the 1820s and did not begin to climb again until the mid-1840s. For nearly twenty years, in other words, supply kept pace with demand as slave prices stayed firmly in the \$250–\$350 range. In the seven years after 1845, however, prices nearly doubled while slave imports averaged only 20 percent of their 1830s level. In the next decade, demand pressures simply burst through the attempted restraints of domestic suppression. Sugar prices almost doubled, and output did not lag far behind. By the early 1860s, slaves were selling routinely for nearly \$1,000, which for prime males meant perhaps \$1,200. The major technological improvements on the plantation came about after the mid-1840s,³⁹ and from 1858 to 1861 slave imports were back up to the peak levels reached in the late 1830s. The drop in slave prices, which stemmed from the American Civil War, probably allowed new abolition legislation finally to become effective in 1866 and 1867.⁴⁰

The economic underpinnings of the two population movements are thus clear enough, and in a broad sense European economic growth fully explains the fluctuations in both flows. While the relationship between industrialization and the end of slavery is still debated, recent interpretations of abolition have stressed the anachronistic light in which slavery came to be viewed after the economic advances of the eighteenth and nineteenth centuries testified to the apparent superiority of self-interest and a free labor market.⁴¹ The conviction of the superiority of free labor on the part of the economically advanced nations was not shaken by the fact that sugar, coffee, and cotton could all be produced more cheaply by unfree labor. First the slave trade and then slavery itself had to be eliminated, despite the growing needs of an industrializing economy for plantation produce. Although the British, French, and American attempts to suppress the transatlantic slave trade (after two centuries of full participation in it) did not at first succeed, suppression when it came and the fluctuations in volume generated in the process were the direct results of outside pressure on the Brazilian and Spanish governments, particularly

³⁸ David R. Murray, *Odious Commerce: Britain, Spain, and the Abolition of the Cuban Slave Trade* (Cambridge, 1980), 181–207.

³⁹ Franklin W. Knight, *Slave Society in Cuba during the Nineteenth Century* (Madison, Wisc., 1970), 30–46; and Manuel Moreno Fraginals, *The Sugarmill: The Socioeconomic Complex of Sugar in Cuba*, trans. Cedric Belfrage (New York, 1976), 81–127.

⁴⁰ In addition to the works on Cuba already cited, data on Cuban slave prices can be found in H. S. Aimes, *A History of Slavery in Cuba* (New York, 1907), 267–68; and Eltis, “The Transatlantic Slave Trade, 1821–1843,” 324. The most complete source for prices of Cuban slaves in the final years of the transatlantic traffic is Fraginals *et al.*, “The Level and Structure of Slave Prices on Cuban Plantations in the Middle of the Nineteenth Century.” Preliminary analysis of these plantation-based data indicates a price trend consistent with fluctuations in imports. Prices rose sharply between 1856 and 1859 before falling back slightly to 1861 and more sharply thereafter to 1863.

⁴¹ See, for example, the work of David Brion Davis as well as Howard Temperley, “Capitalism, Slavery, and Ideology,” *Past & Present*, no. 75 (1977): 94–118; and Stanley L. Engerman and David Eltis, “Economic Aspects of the Abolition Debate,” in Christine Bolt and Seymour Drescher, eds., *Anti-Slavery, Religion, and Reform* (Folkestone, Kent, 1980), 272–93. Few recent writers have pursued Eric Williams’s search for direct economic gains accruing to those governments and classes responsible for abolition.

from the British. This pressure came partly in the form of direct diplomatic representation and partly—and more subtly—through more informal channels as the process of ideological emulation by a developing Spanish and Brazilian bourgeoisie of their more advanced counterparts in Britain and the United States. In the modern era there is no other instance of slave owners and slave traders as a group spontaneously shifting their capital into another line of business.

For the free migrant flow industrialization acted in a different fashion. It generated the social and economic dislocation, and perhaps too the population growth, necessary for the creation of a growing pool of potential migrants,⁴² but it also provided the transportation and communication links between continents without which nineteenth-century migration might not have been very much greater than its eighteenth-century predecessor. Industrialization thus stimulated both movements but eventually brought the African branch to an end.

The two migrations had a second common feature. If the desire to own land remained a central motive of the free migrants, perhaps until the last North American land frontier closed in the 1920s, it was also a major goal for the Afro-American once released from slavery. Everywhere emancipation occurred, the former slave (much to the dismay of European and U.S. abolitionists) attempted to avoid wage labor on or off the plantation. Only on islands such as Barbados, where unoccupied land was unavailable, did the former slave remain on the plantation. This was a pattern, moreover, that held true both for societies where the former slave was overwhelmingly African, as in St. Domingue, and for societies dominated by Creoles. For preindustrial man, whatever his origin, factories and plantations alike were to be avoided unless the wage offered was very high.

Many postemancipation societies were characterized by dual economies where a plantation sector employing indentured Asian labor and oriented to the international market functioned side by side with a village economy supplying domestic markets. In many cases, the former slaves attempted to produce for the international market in much the same way as the farmer of the Midwest, but, with relatively minor exceptions (arrowroot, pimento, and, later, bananas), the tropical and subtropical regions could generate no crop with the sort of small-scale production techniques used on the family and mixed-grain farms of North America. Despite the development late in the nineteenth century of small-scale coffee farming in Brazil and Columbia and of sugar *centrales* in the Caribbean, the former slaves—even with adequate capital and good land—would probably not have been able to compete as family farmers with planters in the production of coffee and sugar. Only in the American South was a partial compromise hammered out in sharecropping and other tenant arrangements.

Freedom of choice is, of course, central to this interpretation. If the aspirations of the preindustrial African and European had apparent similarities, the ability of the former to realize these aspirations was severely circumscribed by slavery and later

⁴² On the relationships between economic and demographic change in England at this time, see most recently E. A. Wrigley and R. S. Schofield, *The Population History of England, 1541–1871* (Cambridge, Mass., 1981).

by discrimination. In most parts of South America and the Caribbean, land tenure laws were designed to discourage small landholdings.⁴³ In the United States and Canada, where small units of relatively cheap land became available in the second half of the nineteenth century, discrimination and the timing of the free migratory flow ensured that Europeans, in particular northern Europeans, would get first choice. Even without institutionalized racial bias, however, it is unlikely that black settlement of western North America could have been extensive. Abolition of slavery in the United States, Cuba, and Brazil occurred as the influx of European peoples reached its zenith. Competition from white immigrants was thus, in itself, a major cause of black immobility. Not until white immigration was closed off did the shift northward and westward of the Southern black labor force in the United States get underway. By then, of course, the land frontier was closed, the aspirations of the migrants had changed, and the direction of the flow shifted to the cities.

THE RELATIVE IMMOBILITY OF AFRICAN DESCENDANTS in the Americas also explains much of the difference in the demographic experience of the two groups. One of the central findings of recent research on vital rates in the Americas has been that the differences between races living in the same regions are small in comparison to the differences between black and white groups combined living in different regions. Life expectancy for both Africans and Europeans was always lower in the tropics than in the temperate regions to the north and south. One reason the Americas were not overwhelmingly peopled by Africans in 1820, which is what we would expect from the migration figures alone, was that far more Africans than Europeans were taken to the tropical regions of the New World. The Europeans, of course, chose to avoid such conditions, but those that did not died at a rate initially higher than that of their African counterparts. Indeed, one reason why Africans rather than Europeans were put to work in the tropical Americas was that the blacks had a higher life expectancy there than did whites. Working conditions certainly had an impact on mortality; sugar plantations were notoriously less healthy than those that produced other plantation crops. But even here regional differences emerged: Louisiana sugar plantations experienced lower mortality rates than their Caribbean equivalents. Black and white trends in fertility also exhibit similarities within regions. In the case of the United States, the rates for the South for both races were quite different from trends in the Northeast and the mid-Atlantic states.⁴⁴

⁴³ For the reaction of exslaves to emancipation, see W. A. Green, *British Slave Emancipation* (Oxford, 1976); Sidney W. Mintz, "Slavery and the Rise of Peasantries," *Historical Reflections*, 6 (1979): 213–53; and Stanley L. Engerman, "Economic Aspects of the Adjustment to Emancipation in the United States and the British West Indies," *Journal of Interdisciplinary History*, 13 (1982): 191–220. For British Guiana, see Alan H. Adamson, *Sugar without Slaves: The Political Economy of British Guiana, 1838–1904* (New Haven, 1972), esp. 34–103; and, for Brazil, see Nathaniel H. Leff, "Economic Retardation in Nineteenth-Century Brazil," *Economic History Review*, 25 (1972): 490–91.

⁴⁴ Robert W. Fogel *et al.*, "The Economics of Mortality in North America, 1650–1910: A Description of a Research Project," *Historical Methods*, 11 (1978): 75–108; Fogel and Stanley L. Engerman, "Recent Findings in the Study of Slave Demography and Family Structure," *Sociology and Social Research*, 63 (1979): 556–89; and B.

The explanation for the broad mortality trends is probably epidemiological. As Philip Curtin argued some years ago, migration exposed individuals to new disease environments to which they had little or no immunity. At the beginning of the European shift to the New World, Europe constituted one large disease environment, and Europeans, on the basis of considerable intercommunication, had acquired a wide range of immunities to that environment.⁴⁵ Much of Africa, however, was a separate environment or, given the less extensive interaction of the inhabitants of the continent, a collection of disease environments. Movements of both Europeans and Africans to the Americas meant the breach of a third self-contained epidemiological zone, and the consequences for the American Indian are well known. The European and American environments became at least somewhat merged fairly quickly as a result of massive interchange between the two areas, but the African zone remained largely separate until the twentieth century. The greater African life expectancy in the tropical New World was based on the immunities to yellow fever and malaria acquired in Africa, but Africans going to the Americas and Europeans and Americans going to Africa, particularly West Africa, could always expect much higher death rates than they would have experienced if they had stayed at home. In addition, the relative isolation of many African societies meant that, for the African slaves, the unfamiliar epidemiological environment began long before the slaves reached the American shore.

These considerations suggest a number of propositions. First, mortality rates for the descendants of the two groups in any given part of the Americas should have been broadly similar, since some of the immunities brought over by Africans died with the protected individuals.⁴⁶ Second, mortality rates for Europeans traveling to the temperate Americas should have fallen over time as the two zones developed a measure of epidemiological homogeneity. Third, the high mortality rates for Africans should have begun even before they embarked on the slave ships. Fourth, persons traveling between those parts of the tropical Americas and Africa that were most alike epidemiologically and that developed the fullest intercommunication should have experienced lower mortality rates than did travelers on other routes. Fifth, the state of medical knowledge at the time was such that the treatment accorded to the two migrant groups during the voyage should have had little or no influence over mortality. What was not known could scarcely have been influential. The first two of these propositions in particular have been broadly established in the literature. Support for the remaining three, with which the remainder of this

W. Higman, *Slave Population and Economy in Jamaica, 1807–1834* (Cambridge, 1976), 99–138. Brazilian slave fertility, though high by European standards, may have lagged behind that of the free population; Slenes, "The Demography and Economics of Brazilian Slavery, 1850–1888," 297–340.

⁴⁵ Much of the following is based on Curtin, "Epidemiology and the Slave Trade." Also see Curtin's "Slavery and Empire," *Annals of the New York Academy of Sciences*, 292 (1977): 3–11; and the works of William H. McNeill, most recently his "Human Migration: A Historical Overview," in William H. McNeill and Ruth S. Adams, eds., *Human Migration: Patterns and Policies* (Bloomington, Ind., 1978), 3–19. The emphasis on the importance of the cities in McNeill's "Historical Overview" is not, however, shared by the interpretation here.

⁴⁶ See Thomas Schick, "A Quantitative Analysis of Liberian Colonization from 1820 to 1843, with Special Reference to Mortality," *Journal of African History*, 12 (1971): 45–60. The genetic base of some immunities, it should be noted, meant that, although blacks and whites often died at similar rates, the causes of death often differed. See Kenneth F. Kiple and Virginia Himmelsteib King, *Another Dimension to the Black Diaspora: Diet, Disease, and Racism* (New York, 1981).

essay is chiefly concerned, may be derived from a review of the physical conditions and the mortality on the transatlantic passage, which the nineteenth-century migrant had to face.

African and European peoples underwent a voyage to the Americas of approximately equal length and involving considerable suffering. By 1872, 92 percent of the passengers arriving in New York came by steamship, and the annualized mortality rate is somewhat below the rate for the American population as a whole—which, given the age structure of the migrants, is as it should have been. Voyage time from Liverpool to New York averaged less than three weeks.⁴⁷ In the first half of the century, however, while the slave trade was still at its height and the sailing ship was used almost exclusively for both free and coerced traffic, the length of time was more than double that, and contemporaries frequently compared conditions in the two. The *Sarah* and the *Dove* left Fort William, in Scotland, for Picton, Nova Scotia, in 1801 with almost 50 percent more passengers than they would have been allowed to carry if they had been carrying slaves for the transatlantic trade,⁴⁸ and parliamentary debates and select committee reports contain many references to similar cases. Between 1807, when the British and American trades were abolished, and the later 1840s, when passenger acts began to have a significant impact, neither the slave nor free migrant traffic was subjected to much effective regulation. As the reformers pointed out, the captain of the migrant ship had no direct pecuniary interest in his passengers once on board—unlike the slave captain who normally received a commission on the value of slaves disembarked. During this period, the human cargoes were rushed on board shortly before departure—slaves to reduce the risk of their becoming prize to the cruisers, free migrants because port regulations forbade lights and fires on board. Voyages for both might take up to seventy days, and neither slaves nor passengers were normally allowed on deck. The crowding below deck often meant that ships in either trade could be recognized at a gun shot's distance by the smell, even in calm weather.⁴⁹

Although the standard works on migration and the slave trade yield abundant evidence of extreme conditions, not all distinguish with precision the differences between the extreme and the typical. A more systematic comparison of ships and cargoes entering Quebec—a route on which conditions were recognized as being as bad as on any Atlantic migrant route—with selected branches of the slave trade is now possible. Table 2 contains the relevant figures. The typical ship carrying passengers to Quebec did not increase in size until after 1850, but these passenger ships were always twice as large as the slaving equivalents. Yet the passenger ships carried on average, however, only one-third as many passengers as the slave ship and accordingly had a passenger-per-ton ratio of only one-eighth to one-tenth of its slave counterpart.⁵⁰ Although ships arriving at Quebec and New York frequently

⁴⁷ U.S. Congress, Senate Executive Document, 43–1 (1874), no. 23: *Report on the Immigration Service*, 12–13, 46–47, 150–51.

⁴⁸ K. A. Walpole, "The Humanitarian Movement of the Early Nineteenth Century to Remedy Abuses on Emigrant Vessels to America," *Transactions of the Royal Historical Society*, 4th ser., 14 (1931): 199–200.

⁴⁹ Great Britain, *Parliamentary Papers [PP]*, 1839, volume 17: *Report on the Affairs of British North America from the Earl of Durham*, app. B, 66.

⁵⁰ As deck space did not increase proportionately with tonnage, this does not mean that slaves were eight to ten times more crowded than migrants.

TABLE 2
Transatlantic Passenger Shipping, 1816–1855
(Tonnage, Voyage Time, Number of Persons, and Persons per Ton)

<i>Year</i>	<i>Average Voyage Time</i>	<i>Average Number of Persons Embarked</i>	<i>Average Tonnage per Ship</i>	<i>Persons per Ton of Vessel</i>
<i>Ships Arriving at Quebec</i>				
1841	44.0 (293)	96.5 (293)	367.5 (1,123) ^a	0.26
1842	45.0 (332)	134.3 (332)	321.4 (739) ^a	0.42
1843	45.0 (279)	78.1 (279)	382.3 (1,121) ^a	0.20
1844	n/a	n/a	383.5 (1,106) ^a	
1845	n/a	n/a	407.9 (1,371) ^a	
1846	47.0 (306)	107.9 (306)	397.4 (306)	0.27
1847 ^b	44.4 (406)	223.3 (406)	308.5 (442)	0.72
1848	42.6 (278)	98.5 (278)	403.1 (278)	0.24
1849	45.0 (347)	110.9 (347)	380.1 (347)	0.29
1850	40.8 (312)	101.3 (213)	n/a (312)	
1851	41.5 (337)	119.2 (337)	440.1 (337)	0.29
1852	39.0 (345)	108.9 (345)	406.7 (345)	0.27
1853	48.0 (324)	110.0 (324)	480.5 (324)	0.23
1854	48.5 (386)	136.9 (386)	528.6 (386)	0.26
1855	44.5 (188)	110.2 (188)	540.8 (188)	0.20
<i>Slave Ships</i>				
CARIBBEAN				
1816–43	48.4 (14)			
1821–30		312.4 (93)	183.5 (69)	2.13 (52)
1831–36		343.3 (56)	169.2 (102)	2.51 (44)
1837–43		348.4 (142)	141.1 (111)	3.00 (46)
RIO DE JANEIRO				
1821–30	34.0 ^c (228)	408.2 (631)	187.0 (13)	2.35 (74)
1831–43	38.0 ^c (50)	431.4 (429)	183.2 (243)	3.00 (51)

^a Includes ships without passengers.

^b Excludes vessels arriving from German ports.

^c Excludes vessels arriving from Southeast Africa.

SOURCES: Great Britain, *Parliamentary Papers*, 1842, 31: 11; *ibid.*, 1843, 34: 10; *ibid.*, 1844, 35: 10; *ibid.*, 1847, 39: 21, and 60: 121; *ibid.*, 1847–48, 47: 21, 39; *ibid.*, 1849, 38: 33; *ibid.*, 1851, 40: 3, 9, 13; *ibid.*, 1852, 32: 27; *ibid.*, 1852–53, 68: 14; *ibid.*, 1854, 46: 27; *ibid.*, 1854–55, 39: 15; and *ibid.*, 1857, 10 (1st sess.): 1, 18; and David Eltis, “The Impact of Abolition on the Atlantic Slave Trade,” in David Eltis and James Walvin, eds., *The Abolition of the Atlantic Slave Trade* (Madison, Wisc., 1981), 158, 162, 164.

carried cargo in addition to passengers (slave ships in this period carried only slaves), the cargo was normally carried in the hold below the steerage decks, where it took the place of ballast in an empty ship; the cargo, in other words, did not reduce the space available to passengers. Indeed, the passenger-per-ton ratio for migrants—apart from the famine year of 1847—shows remarkably little variation through the fifteen years surveyed. The advent of steam in the early 1850s, it should be noted, increased the size of the ship but did little, at least initially, to reduce passenger per ton ratios or voyage lengths. The fluctuations in such figures for the slave trade are much greater (see Table 2), but these variations are largely the result of attempts to suppress the trade.⁵¹

In the first half of the nineteenth century, extreme cases of overcrowding in migrant ships were always likely to occur, but historians have tended to ignore the implications of the cotton trade from New York and the timber trade from Quebec, which meant low freight rates for the less bulky westbound traffic and a scale of shipping capacity that, except possibly for 1847, not even the Irish millions could fill. In most years of the 1840s, only one-quarter to one-half of the ships sailing between British ports and Quebec carried migrants, even though any ship of the period could be quickly adapted to transport steerage passengers. Of those that did carry passengers, many had less than fifty, and the reports of the Canadian agent general for emigration suggest that on average only one-third to two-thirds of the berths permitted under the Passenger Acts were actually filled and that very few ships provided a space smaller than the ten square feet of deck per person required by the 1842 act.⁵²

By comparison, slave ships in the last two decades of the traffic measured on average 85.2 feet in length, 22.2 feet in breadth, and 11.8 feet in depth, which is consistent with a slave deck that cannot have exceeded fifteen hundred square feet and that, given the sharp lines of the typical slaver at this time, was probably less. Since cargoes averaged three hundred and fifty to four hundred slaves, deck space per slave must have been in the three- to four-square-foot range. The depth of these vessels further suggests that the height of the slave deck must have been less than the 5.5 feet required for immigrant shipping by the weakest of the nineteenth-century Passenger Acts. The act of 1799, which regulated the British slave trade until its abolition eight years later, specified 5.0 feet as the minimum height; one appalling though atypical slaver had a space between decks of only 14 inches.⁵³

⁵¹ Eltis, "The Impact of Abolition on the Atlantic Slave Trade," 170–73.

⁵² For example, Terry Coleman implied that conditions on average were worse than the minimum provisions stipulated in the Passenger Acts, and, although Oliver Macdonagh recognized the abundance of shipping capacity, he used contemporary comments on the transatlantic passage to convey the sense that extreme conditions were, in fact, typical; Coleman, *Passage to America: A History of Emigration from Great Britain and Ireland to America in the Mid-Nineteenth Century* (London, 1972), chaps. 12, 13, and app. A; and Macdonagh, *A Pattern of Government Growth: The Passenger Acts and Their Enforcement, 1800–1860* (London, 1961), 43–51. In fact, ships rarely carried more passengers than the acts allowed; see *PP*, 1842, 31: 156; *ibid.*, 1851, 40: 5; and *ibid.*, 1859, 38: 27. For the number and tonnage of ships involved in trade between the Canadas and Britain, see *ibid.*, 1847, 60. These figures should be compared to the same sort of information for passenger shipping in the annual reports of the immigration commissioner in Canada.

⁵³ Data for shipping dimensions are based on the measurements of eighty-five ships made by naval officers and court officials and reported in various volumes of the Foreign Office series. The British Act was 39 George III, c. 80.

Even without attempts to suppress the trade, which tended to worsen conditions on board, the large discrepancy in space per person carried in the two trades would still have existed.

For contemporaries as well as modern commentators, such dramatic differences have provided all the explanations necessary for the striking differences in the mortality rates of the two trades. For the Quebec branch of the migrant traffic, deaths on board ship averaged just under 1 percent of the passengers embarked between 1833 and 1855 and exceeded 1 percent in only four of these years. Even if those who died in quarantine after arrival were included (they are omitted here because the equivalent figures for the slave trade are not available), the rate would average less than 1.5 percent.⁵⁴ In the slave trade, however, the lowest annual rate for traffic to the Caribbean (based on large samples of voyages—346—for the English trade of the 1790s) is 5.65 percent, a figure only just below the highest annual figure ever recorded for the Quebec migrant traffic. From 1821 to 1866, slave mortality on Cuban-bound ships—117 voyages—averaged 17.1 percent, while during the last twenty years of the Rio de Janeiro traffic—53 voyages—the rates were even higher, averaging 18 percent.⁵⁵ Yet at normal person-per-ton ratios neither in the slave trade nor in the migrant trade was there any connection between crowding and mortality.⁵⁶

For the nineteenth century, rather vivid illustration of this point is provided by the experience of British naval officers who, having captured slave ships within hours of their leaving African embarkation points, then had to dispatch the human cargoes to Sierra Leone, a voyage often of five or six weeks in length. These officers, whose humanitarian instincts were encouraged by a bounty in slaves landed alive at Sierra Leone, frequently redistributed the captured cargo among two or even three ships, provided ample food and water, and yet still found the slaves dying in transit at about the same rate that they would have had they proceeded on their way to the Americas.⁵⁷ Voyage time does little to explain slave mortality except in those instances in which voyage length greatly exceeded the expected time for a given route.

In the migrant traffic, the experience was similar. The colonial land and

⁵⁴ Henry Gemery allowed for 10 percent passage mortality, but two of his sources include many cases that involve deprivation or confinement of passengers prior to embarkation and can hardly have been typical; "Emigration from the British Isles to the New World, 1630–1700," 186–87; John Duffy, "The Passage to the Colonies," *Mississippi Valley Historical Review*, 38 (1951): 21–30; and W. A. Knittle, *Early Eighteenth-Century Palatine Emigration* (Philadelphia, 1937), 2–5, 143–48.

⁵⁵ I have calculated the rates for the nineteenth century from a data bank containing information on 5,328 voyages that was collected mainly from the British Foreign Office records (FO 84). For shipboard mortality in the eighteenth century, see Herbert S. Klein and Stanley L. Engerman, "Slave Mortality on British Ships, 1791–1797," in Roger T. Anstey and P. E. H. Hair, eds., *Liverpool, the African Slave Trade, and Abolition* (Liverpool, 1976), 118. For a comparison of slave and free migrant mortality, see Herbert S. Klein, *The Middle Passage: Comparative Studies in the Atlantic Slave Trade* (New Haven, 1978), 90–93. Klein's *Middle Passage* also contains the best synthesis of work on shipboard mortality in the slave trade; *ibid.*, 229–39.

⁵⁶ In separate studies of the French and English eighteenth-century slave trades, Herbert S. Klein and Stanley L. Engerman found no relationship between mortality and the size of the vessels or between mortality and the packing of slaves in a given ship size; see their "Slave Mortality on British Ships, 1791–1797," 118–121, and "A Note on Mortality in the French Trade in the Eighteenth Century," in Gemery and Hogendorn, *The Uncommon Market*, 263–67.

⁵⁷ David Northrup, "African Mortality in the Suppression of the Slave Trade: The Case of the Bight of Biafra," *Journal of Interdisciplinary History*, 9 (1978): 47–64.

emigration commissioners often included in their annual general reports a table showing mortality rates by size of ship and number of passengers carried.⁵⁸ In 1847, the worst year of the century for emigrants sailing to Quebec, data on the 415 ships disembarking passengers at the quarantine island of Grosse Île show no significant correlation between mortality and number of persons carried. Figures for other years are similar. Quebec immigrant mortality shows, moreover, the same large variations among ships that the slave trade does. Of the 415 ships arriving at Quebec in 1847, 108 experienced no deaths and 75 had three or fewer passengers die; many of these vessels carried more than two hundred passengers. As might be expected, the child mortality rate was five to six times the rate for adults. Nor does voyage time explain very much. Mortality on ships arriving in August, when the voyage normally took fourteen days longer than the average time for the year, had rates similar to those on ships arriving earlier in the season.⁵⁹ In terms paralleling the work of modern scholars on the slave trade, the Colonial Land and Emigration Office reported in 1854, after surveying the whole North Atlantic migration movement, "We have endeavoured to extract from these returns some general rules to account for . . . differences in the mortality, but hitherto without success. In time of sailing, ventilation, numbers, space, food and even origin of the emigrants we can discover no probable explanation. . . ."⁶⁰

Mortality was, quite likely, a function of the diseases on board ship when it left port. Friedrich Kapp, one of the New York emigration officers in the 1860s, echoed the views of many associated with the free migrant flow when he named cholera, typhus, and smallpox as the three main diseases of migrants.⁶¹ The records of the quarantine hospital at Grosse Île, Quebec, indicate that between 1833 and 1850 cholera was present only in 1834 and 1849 and that smallpox was a significant killer only in 1837 and 1846. In every year, however, the category "fever" (including typhus) and dysentery claimed the greatest share of deaths and two-thirds of all quarantine deaths in the period 1833–50.⁶² Although this largest category fluctuated considerably from year to year, the peak years for mortality among Quebec arrivals coincide either with the cholera or smallpox years of 1834 and 1837 or with the period 1847–49, when, as a result of famine conditions, the nutritional state of migrants was poor. Cholera and smallpox were obviously extensions of the epidemics current in the British Isles at the time of sailing; dysentery and typhus, however, occurred frequently when large numbers of people were crowded together without adequate sanitation. The lack of correlation between deaths and size of cargo suggests that even half-empty ships generated pathogenic conditions,

⁵⁸ See, for example, *PP*, 1851, 10 (1st sess.): *Despatches Relating to Emigration to the North American Colonies*, 5.

⁵⁹ For the complete list of passenger arrivals in Quebec in 1847, see *PP*, 1847–48, 47: *Papers Relative to Emigration to the British North American Provinces*, 25–31. That voyage length was not critical under normal circumstances is suggested by a mortality rate of less than 1 percent on convict ships to Australia between 1815 and 1868, despite a voyage that was three times as long as that across the North Atlantic; see A. G. L. Shaw, *Convicts and the Colonies: A Study of Penal Transportation from Great Britain and Ireland to Australia and Other Parts of the British Empire* (London, 1966), 116–17.

⁶⁰ U.S. Congress, House Executive Document, 33–1 (1854), no. 116: 5. Also see T. W. C. Murdoch's evidence in *Second Report of the Select Committee on Emigrant Ships*, *PP*, 1854, 13: 8–9.

⁶¹ *Immigration and the Commissioners of Emigration* (New York, 1870), 25–26.

⁶² *PP*, 1844, 35: *Annual Report of the Emigration Agent*, app., 27; and *ibid.*, 1851, 60: *Despatch Transmitting Report from the Chief Agent of Emigration*, 27.

partly because ship captains often used bulkheads to adjust the size of the steerage passenger area to the number of passengers. Of the two diseases named in the category of fever and dysentery, typhus had the higher mortality, but a striking feature of Grosse Île's returns is the low ratio of deaths to admissions for fever and dysentery. Of the 5,352 admissions from 1833 to 1850 (excluding 1847, for which no breakdown exists), only 454 or 8.5 percent of the patients died. For smallpox the ratio was even lower. For cholera, however, about 70 percent of those admitted in 1834 died.⁶³

The equivalent of Grosse Île for the coerced traffic of the 1840s was the center for liberated Africans near Jamestown, St. Helena. This site was selected by the British government as the disembarkation point of captured slave vessels, partly because of the high mortality on slavers taken to Sierra Leone and partly because of the trade's shift southward in these years. The relocation of the center from Sierra Leone to St. Helena certainly resulted in a sharp drop in the time that the recaptured slaves spent at sea. Although the former was on the mainland and the latter was one-third of the way to the Americas, the prevailing winds and position of the major export points meant that St. Helena could be reached on average in less than one-third of the sailing time required for a captured slaver to make Sierra Leone. Yet the recaptives continued to die at the same or even increased rates. By 1849, over 30 percent of the 12,405 slaves who disembarked at Jamestown since 1840 had died there, despite the transshipment of Africans to the West Indies within a few months of their arrival at St. Helena.⁶⁴ Thus, it appeared to matter little whether or not the slaves were on board ship or, given the 20 percent mortality rate among recaptives landed at the Cape of Good Hope,⁶⁵ at what port they disembarked. Indeed, it is hard to imagine that the mortality rate among those who made it to the Americas without being intercepted was higher than that experienced by the recaptives deposited at St. Helena.⁶⁶ Moreover, when many of the recaptives subsequently emigrated from Sierra Leone and St. Helena to British colonies in the Americas under British Passenger Act regulations, which were the same as or more stringent than those for the North American migrant, mortality during the crossing was 7.4 percent,⁶⁷ much closer to slave trade than to free migrant rates.

The several medical inquiries stimulated by the revelation of the St. Helena

⁶³ I have calculated these figures from the total mortality statistics on the assumption that mortality rates from diseases other than cholera were the same in 1834 as those from 1835 to 1846. For the mortality statistics, see *PP*, 1851, 40: 9.

⁶⁴ Colonial Office to Palmerston, October 8, 1849, with enclosures, and November 27, 1849, with enclosures, both in FO 84, 780.

⁶⁵ Colonial Office to Palmerston, October 8, 1849, sub enclosure "Statement of Deaths, November 19, 1847, to October 18, 1848," FO 84, 780.

⁶⁶ Asiegbu, *Slavery and the Politics of Liberation, 1787–1861*, 130–35. British cruisers off the Brazilian coast captured 1,275 slaves on four ships in April 1839. These Africans, all taken from south of the equator, were placed almost immediately under the supervision of the Rio de Janeiro Court of Mixed Commission. Within six weeks, 147 (11.5 percent) had died, over half from dysentery; W. G. Ouseley to Palmerston, June 6, 1839, enclosure from R. Hesketh, May 31, 1839, FO 84, 286. Allowing for 10 percent mortality on the crossing (the actual figures were not reported) and for further deaths after the first six weeks in Brazil, it is probable that the mortality rate was similar to that experienced by the Africans landed at St. Helena.

⁶⁷ I have calculated these figures from the returns of thirty-four ships from 1847 to 1849: see Asiegbu, *Slavery and the Politics of Liberation, 1787–1861*, 130–35. The St. Helena center for liberated Africans was ordered closed in 1854, partly because of the high mortality figures.

figures were referred to the colonial land and emigration commissioners for evaluation. "[T]he great mortality," they reported, "appears to arise from a very intractable and offensive form of Dysentery called by the natives Maculla attacking more peculiarly males and especially those from particular countries. . . . [O]ther diseases of course exist, but Ophthalmia is the only one which can be said to be prevalent . . . [and this latter has] no connexion with the mortality."⁶⁸ The main cause of death was thus the same in the 1840s as it had been in the previous century. Dysentery, which while common resulted in a fairly low incidence of mortality among European shipboard migrants, slaughtered the African in the South Atlantic; cholera, smallpox, and the louse-borne typhus, however, were rarely found in the slave trade, in the case of typhus, no doubt, because the slaves were typically naked on board.⁶⁹ There is no way of discovering whether the dysentery was amoebic, which is endemic in parts of Africa, or bacillary, which is endemic in Europe but epidemic in Africa, but the British inquiries of the 1840s do make clear that dysentery was as prevalent among the slave barracoons on the coast as on board ship: "[N]egroes usually suffer most shortly after being brought down from the interior," noted one Angolan-based report, and, "during the first three or four months after being brought down, the greatest care is requisite."⁷⁰ Those Africans from relatively isolated disease environments risked death long before they reached the Americas or even the slave ship that was to take them there.⁷¹

This epidemiological approach to migration and mortality yields some insight into the striking differences in shipboard mortality between African regions, variations that were apparent in both the eighteenth and nineteenth centuries. Ships leaving ports of the Bight of Biafra, for example, always experienced much greater losses than those from ports of the Bight of Benin.⁷²

Declining rates over time seem to have been prevalent where pairs of regions across the Atlantic developed the closest intercommunication. In addition, low rates in one region relative to another perhaps occurred because the former was supplied by hinterlands that were the most fully integrated economically, socially, and, by implication, epidemiologically. The Bight of Benin–Bahia route, which had the lowest mortality rate of any branch of the slave trade in the nineteenth century, combined these characteristics. Not only were African societies in general and the

⁶⁸ Colonial Office to Palmerston, November 27, 1849, with enclosure from Thomas C. Murdoch and Frederic Rogers to Earl Grey, August 25, 1849, FO 84, 780. Copies of three separate reports or commentaries on the St. Helena situation are included under this covering letter.

⁶⁹ Inoculation was practiced in parts of Africa before European contact was established; Eugenia W. Herbert, "Smallpox Inoculation in Africa," *Journal of African History*, 16 (1975): 539–59. It was also practiced by European slave traders subsequently; Klein and Engerman, "A Note on Mortality in the French Slave Trade." The *Rosita*, alias *Esperanza*, nevertheless lost most of her cargo from smallpox in 1859; E. M. Archibald to Russell, September 23, 1859, FO 84, 1086. For a discussion of disease on the Middle Passage, see Klein, *The Middle Passage*, 200–02.

⁷⁰ Colonial Office to Palmerston, May 17, 1849, enclosure from George Brand, British commissioner at Loanda, November 20, 1848, FO 84, 779.

⁷¹ For the importance of mortality before the slave embarked on the African coast, see Joseph C. Miller, "Mortality in the Atlantic Slave Trade: Statistical Evidence on Causality," *Journal of Interdisciplinary History*, 11 (1981): 385–423.

⁷² For long-term shipboard mortality trends, see Curtin, *The Atlantic Slave Trade*, 276–77; and Robert Louis Stein, *The French Slave Trade in the Eighteenth Century: An Old Regime Business* (Madison, Wisc., 1979), 99–100. For the high mortality among slaves from ports in the Bight of Biafra, see Klein and Engerman, "Slave Mortality on British Ships, 1791–1797"; and Northrup, "African Mortality in the Suppression of the Slave Trade."

Yoruba peoples in particular mobile and highly dependent on internal trade routes, but the African impact on Bahia and the Bahian impact on the Bight of Benin was certainly greater than the interchange between any other two regions of Africa and the Americas.⁷³ Citizenship in Dahomey, a major slave supply state in this region, was based not on kinship but "simply on a willingness and ability to serve the king," with the result that non-Aja peoples, including Europeans, became Dahomeans and the population of Abomey, the capital, was cosmopolitan.⁷⁴

In the Bight of Biafra, however, European shore-based establishments were rare, and ships trading there began their voyages in a wide variety of European and American ports. Even in the nineteenth century, trading was conducted from the ship, and slaves remained under African control until the cargo was complete. Although internal trade networks were well developed, there were no equivalents to the Portuguese-Brazilian trading communities or the feedback from the Americas in this region.⁷⁵ In Angola, from whence came most of the St. Helena recaptives, population densities were lower and long-distance trading relationships were perhaps less intense and ubiquitous than in the slave coast region of West Africa. There might well have been a relationship between mortality and the volume of exports, as traders drew on societies ever more remote from the coast. Yet, although the level of the slave trade from Angola in the 1840s was very high, it does not seem sufficiently greater than earlier peaks to explain the very high mortality of that decade, and there is certainly room for interpretations that focus on climatic change, drought, and famine. But, as the crisis of the 1840s preceded abolition, it cannot have been the consequence of demographic pressures that built up subsequent to the ending of the emigration of people.⁷⁶ Thus, in the nineteenth century the European migrants died either because of unsanitary conditions or epidemics that were likely to occur anywhere in Europe or North America when large numbers of people were crowded together, and African migrants died because movement brought them into contact with a new disease or, perhaps, a new form of an old disease.

Understanding these differences allows us to return to the question of why the nineteenth-century Americas were predominantly white, despite the vastly greater

⁷³ Pierre Verger, "Rôle joué par le tabac de Bahia dans la traite des esclaves au golfe du Benin," *Cahiers d'études africaines*, 15 (1965): 349–69. For European communities on the adjacent Gold Coast, see Margaret Priestley, *West African Trade and Coastal Society: A Family Study* (London, 1969); and George Nørrregard, *Danish Settlements in West Africa, 1658–1850*, trans. S. Mammen (Boston, 1966). For mortality rates, see Eltis, "The Transatlantic Slave Trade, 1821–1843," 86–93. Brazilian purchasers of Africans were convinced that slaves from the Bight of Benin had greater resistance to disease than those from Angola; A. J. R. Russell-Wood, *Fidalgos and Philanthropists: The Santa Casa da Misericórdia of Bahia* (Berkeley and Los Angeles, 1968), 51. This might account for the price differential for slaves from these two regions.

⁷⁴ I. A. Akinjogbin, *Dahomey and Its Neighbours, 1708–1818* (Cambridge, 1967), 25–26; and Colin W. Newbury, *The Western Slave Coast and Its Rulers* (Oxford, 1961), 6.

⁷⁵ For trading prices in the Bight of Biafra, see Eltis, "The Transatlantic Slave Trade, 1821–1843," 114–65.

⁷⁶ For suggestions of a direct relationship between shipboard mortality and the volume of slave exports, see Miller, "Legal Portuguese Slaving from Angola: Some Preliminary Indications of Volume and Direction, 1760–1830," *Revue française d'histoire d'outre-mer*, 52 (1975): 157–59; and Klein, *The Middle Passage*, 87. It is noteworthy that the Congo and equatorial regions in particular saw very high rates of mortality among forced migrant laborers in the colonial period; see V. Thompson and R. Adloff, *The Emerging States of French Equatorial Africa* (Stanford, 1960), 142; and Lord Hailey, *An African Survey* (London, 1938), 1590. For export trends from Angola, see Miller, "Legal Portuguese Slaving from Angola," 146–51, 160–63; and David Eltis, "The Export of Slaves from Africa, 1821–1843," *Journal of Economic History*, 37 (1977): 412–13, and "The Direction and Fluctuation of the Transatlantic Slave Trade, 1844–1867."

TABLE 3
Cumulative Net Immigration into and Populations of
Selected American Regions, ca. 1820

	<i>Immigration to 1820</i>		<i>Population in 1820</i>	
	<i>African</i>	<i>European</i>	<i>Black and Free Colored</i>	<i>White</i>
UNITED STATES	550,000	651,000	1,771,656	7,866,797
BRITISH WEST INDIES	1,600,000	210,000	839,000	57,000 ^a
FRENCH, DANISH, AND DUTCH WEST INDIES ^b	2,235,000	254,000	814,600	73,600
BRAZIL ^c	2,942,000	500,000	2,660,000	920,000
SPANISH AMERICA, EXCLUDING PERU ^c	1,072,000	750,000	5,150,000 ^d	3,429,000

^a The figure for the white population of the British West Indies is as of the year 1829 and includes an estimate for Jamaica.

^b The St. Domingue component of these data is for the year 1791.

^c The figures for Brazil and Spanish America are for all years through 1825.

^d Angel Rosenblat described this group as "mestizos and mulattos"; hence, the African component may be taken as a small fraction of this figure.

SOURCES: H. H. S. Aimes, *A History of Slavery in Cuba* (New York, 1907), 269; Philip D. Curtin, *The Atlantic Slave Trade: A Census* (Madison, Wisc., 1969), 35, 68, 79–80, 116, 119, 207, 235; Seymour Drescher, *Econocide: British Slavery in the Era of Abolition* (Pittsburgh, 1977), 173; Robert William Fogel and Stanley L. Engerman, *Time on the Cross: The Economics of American Negro Slavery*, 1 (Boston, 1974): 25; David W. Galenson, *White Servitude in Colonial America: An Economic Analysis* (New York, 1981), app. 8; Great Britain, *Parliamentary Papers, 1831*, 19: 173, and *1833*, 26: 474–77; Moreau de Jonnés, *Recherches statistiques sur l'esclavage colonial et sur les moyens de le supprimer* (Paris, 1842), 16–42, 49–50; R. R. Kuczynski, *Population Movements* (Oxford, 1936), 102–03; Angel Rosenblat, *La Población indígena y el mestizaje en America*, 2 (Buenos Aires, 1950): 36; Richard B. Sheridan, "Slave Demography in the British West Indies and the Abolition of the Slave Trade," in David Eltis and James Walvin, eds., *The Abolition of the Atlantic Slave Trade* (Madison, Wisc., 1981), 274–75, 279; and U. S. Bureau of the Census, *Historical Statistics of the United States*, 1 (Washington, 1957): 14. For European immigration data, see text.

number of African arrivals. First, life expectancy for all inhabitants of the tropical Americas, to which Africans were taken, was always lower than that of the temperate zones. Second, by the nineteenth century, and probably for some time before, Africans arriving in the Americas could expect to die at a much greater rate than their European counterparts simply because Africa remained an epidemiologically distinct region. "Seasoning" rates, which tended to be similar or to favor the African migrant in the early stages of settlement, came to diverge rather markedly. Although African immunities to tropical fevers gave the African migrant an initial advantage over the European in the tropics, the "seasoning" mortality experienced by Africans on leaving their native environment did not decline very much in the era of the slave trade, but European "seasoning" deaths—at least in the temperate Americas—fell markedly over the same period.⁷⁷

⁷⁷ There remains the possibility that the migrant was free and had higher expectations and thus a greater will to live than did the slave. Although there is some literary evidence of death from psychic shock and suicide, the full record is far from clear on the point. For the medical men of the 1840s who treated the recaptured slaves, the problem was not one of diagnosing the disease but, rather, of finding causes for an affliction the symptoms of which made it relatively easy to identify. For the literary evidence, see, for example, William Pierson, "White

The implications of these differences of transatlantic migration and of resident immigration are illustrated in the comparative immigration and population statistics shown in Table 3. Although the data for Iberian America must remain highly tentative and although they reflect political rather than natural boundaries, the ratios between regions and races are reasonably accurate. These data lend credence to the three broad hypotheses argued here. First and most obvious is the enormous imbalance between black and white migration down to the first half of the nineteenth century. Second is the basic similarity of the demographic experience of blacks and whites within regions. Thus, relatively small numbers of African and European migrants were responsible for the large black and white U.S. population of 1820, but the reverse is apparent, again for both peoples, in the British and French West Indies. And third is the discrepancy for those regions where Europeans apparently increased more rapidly than Africans, a difference that can be explained by the timing of the two population shifts and by the "seasoning" differentials along with the epidemiological trends that occasioned them. Thus, in Brazil 40 percent of the African immigrants arrived in the fifty years prior to 1825 compared to perhaps 3 percent of the European arrivals in the same period. Sex and age imbalances and a high "seasoning" death rate ensured a rapid decrease in the black population relative to its white counterpart, since the Europeans developed a normal population pyramid and epidemiological equilibrium long before 1825. These same factors also help explain the apparently superior demographic performance of U.S. whites relative to blacks. Over half of the U.S.-bound Africans disembarked between 1780 and 1820, whereas over 95 percent of the European migrants had arrived before 1780. We might conclude that, once African arrivals had survived the "seasoning" period and had begun to establish a normal distribution in sex and age, they might expect to experience a natural rate of population growth—comparable to that of whites living in the same region.

THE EXPERIENCE OF AFRICANS AND EUROPEANS IN THE NEW WORLD is dominated, of course, by the bondage of the blacks and the free status of the whites on arrival. Their subsequent experience contained similarities that go beyond the demographic. Indeed, anyone attempting an intercontinental overview of the consequence of these population shifts must recognize that the key differences in cultural trends are those between continents, not those between peoples of different origin on the same continent. The sex ratios and age distribution of slave and free migrant arrivals to the New World between 1820 and 1850—and probably earlier—were very similar, and, although nuclear family groupings existed only in the free migrant traffic, the nuclear or at least the two-parent family quickly emerged in the American slave communities.⁷⁸

Cannibals, Black Martyrs: Fear, Depression, and Religious Faith as Causes of Suicide among Slaves," *Journal of Negro History*, 62 (1977): 147–59.

⁷⁸ R. Graham, "Slave Families on a Rural Estate in Colonial Brazil," *Journal of Social History*, 9 (1976): 382–402; Slenes, "The Demography and Economics of Brazilian Slavery, 1850–1880," 412–83; Michael Craton, "Changing Patterns of Slave Families in the British West Indies," *Journal of Interdisciplinary History*, 10 (1979): 1–35; and B. W. Higman, "African and Creole Slave Family Patterns in Trinidad," in Margaret E. Crahan and Franklin W. Knight, eds., *Africa and the Caribbean: The Legacies of a Link* (Baltimore, 1979), 41–64.

In nutritional trends as well as in vital rates, the shared experience predominates. Second-generation descendants of free migrants and slaves in all parts of the Americas for which data exist were taller than their parents, and genetic traits cannot explain these differences or the differences between American regions. In eighteenth- and nineteenth-century North America, whites were slightly taller than blacks, but, again, these differences were small compared to those on either side of the Atlantic. American-born whites were 2 to 4 inches taller than their English counterparts about 1760 but only 1.3 inches taller than American slaves. And American blacks were, in turn, substantially taller than their African forbears. One hundred years later, the gap between U.S. whites and U.S. blacks had narrowed, but that between North America on the one hand and Europe and Africa on the other remained. Europe experienced a strong secular increase in heights later in the nineteenth century, but Africa has yet to show a similar trend so that Afro-Americans are significantly taller than Africans. The interesting question, of course, is why such a trend developed. In the terms of the earlier discussion, was it because of the availability of food (and income) in the New World (people had previously wished to consume more but had been unable to do so) or was it because of an increase in the amount of work (in the case of slaves, a forced increase) to achieve more in the way of pecuniary as opposed to nonpecuniary goals?⁷⁹

Answers to this and similar questions generated by the findings of the last decade are of greater intrinsic interest than the findings themselves. Although the gaps in our knowledge of the volume, vital rates, family patterns, and nutritional experiences of the transatlantic migrants and their descendants are obvious, particularly for the Hispanic and Luso Americas, enough is known to generate new interpretations of the population shift, particularly by those with a penchant for the paradox and the moral dilemma.⁸⁰ The development of capital within Europe, its expansion to the Americas, and the attendant migration of millions of Africans and Europeans created a complex mix of costs and benefits for those involved. To explore and make sense of this mix within the boundaries established by recent quantitative work are tasks more difficult but also more important than the effort that has produced the evidence surveyed here.

⁷⁹ Robert W. Fogel *et al.*, "The Economic and Demographic Significance of Secular Changes in Human Stature: The U.S., 1750–1960" (unpublished paper, 1979); B. W. Higman, "Growth in Afro-Caribbean Slave Populations," *American Journal of Physical Anthropology*, 50 (1979): 373–85; David Eltis, "Nutritional Trends in Africa and the Americas: Heights of Africans, 1819–1839," *Journal of Interdisciplinary History*, 12 (1982): 453–75; and Robert A. Margo and Richard H. Steckel, "Heights of American Slaves: New Evidence on Slave Nutrition and Health," *Social Science History*, 6 (1982). Impressionistic evidence of the similarity of the North American and Brazilian experience, for slaves at least, emerges from the comments of British abolitionists who, after visiting coffee estates and mines in Rio de Janeiro and Minas Gerais, wrote that the slaves were "well fed and exceedingly well worked"; Charlotte Pilkington, Rio de Janeiro, July 27, 1840, and George Pilkington, September 17, 1840, Rhodes House Library, Oxford, file G 79, British Empire MSS, S22. That a similar process occurred in St. Domingue without the aid of the rapid economic development of North America is suggested by the height trends in Haiti; see Jean Fouchard, *The Haitian Maroons: Liberty or Death*, trans. A. Faulkner Watts (New York, 1981), 177. For recent discussions of irregular rainfall and precarious food supplies in much of West Central Africa down to the twentieth century, see Jill R. Dias, "Famine and Disease in the History of Angola, c. 1830–1930," *Journal of African History*, 22 (1981): 349–78; and Joseph C. Miller, "The Significance of Drought, Disease, and Famine in the Agriculturally Marginal Zones of West-Central Africa," *ibid.*, 23 (1982): 17–61.

⁸⁰ For one well-known example, see Edmund S. Morgan, *American Slavery, American Freedom* (New York, 1975).

The Adjustment of Italian Immigrants in Buenos Aires and New York, 1870–1914

SAMUEL L. BAILY

AMEDEO CANUTI AND GIUSEPPE TRIMORA left Italy for the Americas at the turn of the past century. Amedeo was born in the tiny Adriatic fishing village of Sirolo in 1865. When the town's fishing industry came upon hard times during the latter part of the nineteenth century, he and his three brothers went to Buenos Aires. In 1894 Amedeo was recorded in the membership lists of a leading mutual aid society as a literate, twenty-nine-year old sailor who lived with six fellow villagers at 175 Calle La Madrid in the Boca, the center of Argentina's maritime industry. He later married and moved with his family to the nearby town of Quilmes, where he became the owner of a grocery store. Giuseppe's background was somewhat different. As an unmarried Sicilian of thirty-two, he migrated to New York in 1896. The census of 1900 records him as living with eighty other individuals, all but five of whom were Italian, in a tenement house at 228 Elizabeth Street in the heart of one of the city's many Sicilian colonies. Giuseppe could not read, write, or speak English. He listed his occupation as laborer, but at the time of the census he had been unemployed for several months.¹

Amedeo and Giuseppe were joined by millions of their countrymen who migrated to Argentina and the United States before World War I. Some remained permanently in their adopted country. Others returned home. Still others went back and forth many times. By 1914, however, nearly a million Italians lived in Argentina and a million and a half in the United States. In each country, the Italian immigrants settled primarily in the urban areas, especially in the rapidly growing commercial and industrial port cities of Buenos Aires and New York. When World War I broke out, three hundred and twelve thousand Italians—one-third of the total in Argentina—lived in Buenos Aires, and three hundred and seventy

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¹ Alfredo Canuti, Interview with Samuel L. Baily, Numana, Italy, May 12, 1980; Colonia Italiana, "Libri d'iscrizione dei soci, 1894," Archiv d'Unione e Benevolenza, Buenos Aires; and U.S. Federal Census Schedules, Manuscript, 1900, New York City, enumeration district 128, A2, B2.

thousand Italians—one-quarter of the total in the United States—lived in New York.

What happened to Amedeo, Guiseppe, and the hundreds of thousands of Italians who went to Buenos Aires and New York at the turn of the century is the central focus of this essay. As these immigrants looked for jobs and housing and attempted to make a better life for themselves and their children, how did they interact with each other and with those already living in the two cities? What were the similarities and differences in the patterns of adjustment? And why?²

Scholars in various disciplines have devoted considerable attention to Italian migration to the Americas.³ Although this cumulative scholarly effort has increased our understanding of the process of Italian migration, it has fallen short in several important respects. Among the most important, the coverage of the subject is uneven. Much more has been written about the United States and Italy than about Argentina, Brazil, or Canada. The literature on the United States, moreover, is not only extensive but also more developed in terms of local and topical studies. A second shortcoming is that this work, with few exceptions, is not comparative. Although the Italians immigrated in substantial numbers to several American countries, few scholars have analyzed more than one receiving country. Furthermore, many students of Italian migration have focused on isolated parts of the subject, unmindful of the interrelatedness of the various aspects of the experience. Some, for example, have written exclusively about the receiving society as though nothing of importance had happened before the immigrants got off the boat. Others have accounted for the development of the process solely in economic terms, with little or no mention of social, cultural, psychological, political, or other noneconomic variables. Finally, authors have frequently confused their readers by using such broad, imprecise, and in general poorly defined terms as assimilation, absorption, and integration.

My own work during the past decade and a half has impressed upon me the

² In this essay, I am dealing with the Italians as a group, not as individuals. Amedeo Canuti and Giuseppe Trimora illustrate some of the characteristics of their respective groups of Italian immigrants, but certainly not all.

³ The literature on Italian migration is extensive. Some representative works on Italians in the United States and Argentina include Robert F. Foerster, *The Italian Immigration of Our Times* (Cambridge, Mass., 1919), and "The Italian Factor in the Race Stock of Argentina," *Quarterly Publications of the American Statistical Association*, 16 (1919): 347–60; Luciano J. Iorizzo and Salvatore Mondello, *The Italian Americans* (New York, 1971); Alexander DeConde, *Half Bitter, Half Sweet: An Excursion into Italian American History* (New York, 1971); Silvano M. Tomasi and Madeline H. Engel, eds., *The Italian Experience in the United States* (New York, 1970); Humberto S. Nelli, *Italians in Chicago, 1880–1930* (New York, 1970); Andrew F. Rolle, *The Immigrant Upraised: Adventurers and Colonists in an Expanding America* (Norman, Okla., 1968), and *The Italian Americans: Troubled Roots* (New York, 1980); Edwin Fenton, *Immigrants and Unions—A Case Study: Italians and American Labor, 1870–1920* (New York, 1975); George E. Pozzetta, "The Italians of New York City, 1890–1914" (Ph.D. dissertation, University of North Carolina, 1971); Carla Bianco, *The Two Rosetos* (Bloomington, Ind., 1974); Leonard Covello, *The Social Background of the Italo-American School Child* (Totowa, N.J., 1972); Thomas Kessner, *The Golden Door: Italian and Jewish Immigrant Mobility in New York City, 1880–1915* (New York, 1977); Grazia Dore, *La Democrazia italiana e l'emigrazione in America* (Brescia, 1964); Gino Germani, *Política y sociedad en una época de transición* (Buenos Aires, 1962), and "Mass Immigration and Modernization in Argentina," in Irving Louis Horowitz, ed. *Masses in Latin America* (New York, 1970), 289–330; Antonio Franceschini, *L'Emigrazione italiana nell'America del Sud* (Rome, 1908); Emilio Zuccarini, *I Lavori degli italiani nell Repubblica Argentina* (Buenos Aires, 1910); Carl Solberg, "Mass Migrations in Argentina, 1870–1970," in William H. McNeill and R. S. Adams, eds., *Human Migration* (Bloomington, Ind., 1978), 146–70; Gianfausto Rosoli, ed., *Un Secolo di emigrazione italiana* (Rome, 1978); and Ercole Sori, *L'Emigrazione italiana dall'Unità alla Seconda Guerra Mondiale* (Bologna, 1979).

difficulty of grasping the complexity of the Italian migration experience.⁴ The literature to date provides a base upon which to build, not only because it contains specific information but also because the shortcomings suggest new approaches and directions for future study. Several recent works, whose authors have already approached the subject in new ways, are particularly useful, and I gratefully acknowledge my indebtedness to them.⁵ My intent here is to provide a broad comparative overview of one phase of the process of Italian migration, the immigrants' adjustment to Buenos Aires and New York, to identify the important explanatory variables in the process, and to suggest a framework or model that can be applied to immigrant adjustment in other cities.

Because adjustment is an early phase of assimilation, the meaning of "assimilation" must be established. Scholars have, however, used the term in different ways.⁶ Is it a process or a fixed condition? How does it differ from absorption or integration? When does it begin or end? How can it be measured? And what is its outcome? I use the term "assimilation" to refer to the long-term, open-ended process of interaction that begins when the immigrants first come into contact with a new environment and with the people living there. Adjustment is the first phase of this process, in which the immigrants develop the knowledge, skill, and organization that enable them to function effectively. The advantage to focusing on adjustment lies in the ease with which it can be identified and in the ability to measure it accurately by how quickly and how easily immigrants are able to find housing and employment, to improve their circumstances, and to develop organizations to protect their interests.

ALMOST ALL OF THE ITALIANS who went to Buenos Aires and New York did so primarily to earn more money than they had in Italy—either to send back home or to improve their living conditions in the New World, or perhaps a combination of both. To earn more, however, they first had to find work and housing. The

⁴ This essay forms part of a larger study in progress on Italians in Buenos Aires and New York City during the period of mass migration from 1875 to 1925. For some of my thoughts on the subject, see "The Italians and Organized Labor in the United States and Argentina, 1880–1910," *International Migration Review* [hereafter, *IMR*], 1 (1967): 55–66; "The Italians and the Development of Organized Labor in Argentina, Brazil, and the United States, 1880–1914," *Journal of Social History*, 3 (1969): 123–34; "The Role of the Press and the Assimilation of Italians in Buenos Aires and São Paulo, 1893–1913," *IMR*, 12 (1978): 321–40; "Marriage Patterns and Immigrant Assimilation in Buenos Aires, 1882–1923," *Hispanic American Historical Review*, 60 (1980): 32–48; "Chain Migration of Italians to Argentina: Case Studies of the Agnonesi and the Sirolesi," *Studi Emigrazione* (Rome), 19 (1982): 73–91, and "Las Sociedades de ayuda mutua y el desarrollo de una comunidad italiana en Buenos Aires, 1858–1918," *Desarrollo Económico* (Buenos Aires), 21 (1982): 485–514.

⁵ Josef J. Barton, *Peasants and Strangers: Italians, Rumanians, and Slovaks in an American City, 1890–1950* (Cambridge, Mass., 1975); Rudolph M. Bell, *Fate and Honor, Family and Village: Demographic and Cultural Change in Rural Italy since 1800* (Chicago, 1979); John W. Briggs, *An Italian Passage: Immigrants to Three American Cities, 1890–1930* (New Haven, 1978); Robert F. Harney, "Ambiente and Social Class in North American Little Italies," *Canadian Review of Studies in Nationalism*, 2 (1975): 208–24; Frank Sturino, "Inside the Chain: A Case Study of South Italian Migration to North America, 1880–1930" (Ph.D. dissertation, University of Toronto, 1981); Mark D. Szuchman, *Mobility and Integration in Urban Argentina: Córdoba in the Liberal Era* (Austin, Texas, 1980); and Rudolph J. Vecoli, "Contadini in Chicago: A Critique of *The Uprooted*," *Journal of American History*, 51 (1964): 404–17.

⁶ For a useful evaluation of the literature on assimilation, see Charles Price, "The Study of Assimilation," in J. A. Jackson, ed., *Migration* (New York, 1969), 181–237. Also see S. N. Eisenstadt, *The Absorption of Immigrants* (London, 1954); Milton M. Gordon, *Assimilation in American Life* (New York, 1964); Gino Germani, "Assimila-

temporary migrant, who was interested in maximizing his short-term savings, was willing to live in the least expensive housing, no matter how crowded or unhealthy, and to accept any job.⁷ Those who decided to remain for some time, however, sought to improve their living and working conditions and to form organizations to protect their interests.⁸ Here I am focusing primarily on the permanent migrant. The cumulative search by thousands of Italians in Buenos Aires and New York for jobs and housing, for better living and working conditions, and for collective forms of protection created patterns that can be studied and compared. To the degree that Italians in one city obtained better positions, experienced more occupational and residential upward mobility, and formed more effective community organizations than those in the other city, they "adjusted" more rapidly and successfully to their new environment. Just what were these "patterns"?

Buenos Aires and New York were the leading ports and economic centers of their respective countries at the turn of the century. Although Buenos Aires was a provincial town of one hundred and eighty thousand inhabitants in 1869, it became during the next forty-five years a major metropolis with a population of one million six hundred thousand. In 1870, New York was already a leading industrial and commercial city with a population of just under a million people. By 1914, it had grown to more than five million.⁹ The success of the Italians in their economic pursuits in these two cities is an important indicator of adjustment.

Foreign workers and owners dominated the commercial and industrial sectors of the Buenos Aires economy. As early as 1887, Italians, who accounted for 32 percent of the population, made up 53 percent of the workers in industry, 57.5 percent of the owners of industrial establishments, 39 percent of the workers in commerce, and 16 percent of the owners of commercial establishments. Native Argentines, who made up 47 percent of the population, represented only slightly more than 20 percent of both workers and owners in commerce, 16 percent of the workers in industry, and less than 10 percent of the owners of industrial establishments. During the next three decades, Italian workers and owners in industry gradually declined to between 35 and 40 percent, largely because second-generation Italians were recorded as Argentines. In commerce, however, although the percentage of Italian workers remained the same, that of owners more than

tion of Immigrants in Urban Areas," *Working Papers*, Instituto Torcuato Di Tella (Buenos Aires, 1966); John Goldlust and A. H. Richmond, "A Multivariate Model of Immigrant Adaptation," *IMR*, 8 (1974): 193–227; J. J. Mangalam and H. K. Schwarzweller, "General Theory in the Study of Migration: Current Needs and Difficulties," *ibid.*, 3 (1968): 3–17, and "Some Theoretical Guidelines toward a Sociology of Migration," *ibid.*, 4 (1970): 5–20.

⁷ To avoid the cumbersome he/she, him/her, and his/hers, I use he, him, and his throughout to refer to any individual immigrant, either male or female.

⁸ Many scholars make the distinction between temporary migrants (sojourners) and permanent migrants (immigrants). The definition of these terms is, however, often imprecise. I use "temporary migrant" to refer to those migrants who left Italy with the intention of returning or who returned within five years. "Permanent migrant" refers to those who left Italy with the intention of remaining abroad or who did, in fact, remain there for more than five years. See J. D. Gould, "European Inter-Continental Emigration—The Road Home: Return Migration from the U.S.A.," *Journal of European Economic History*, 9 (1980): 41–112; and Betty Boyd Caroli, *Italian Repatriation from the United States, 1900–1914* (New York, 1973).

⁹ Ira Rosenwaike, *Population History of New York City* (Syracuse, N.Y., 1972), 63, 93, 102; and Guy Bourd  , *Urbanisation et immigration en Am  rique Latine: Buenos Aires* (Paris, 1974), 174.

doubled. Throughout the period, the percentage of Italian immigrants active in these areas stayed above that of native Argentines.¹⁰

In New York, the situation was fundamentally different in large part because the Italians there formed such a small percentage of the total population and, as such, had little chance of dominating any sector of the economy. Although Italians never represented as large a percentage of workers or owners in industry and commerce as did native Americans, they formed a substantial proportion of the workers in personal and domestic service. In 1900, when they accounted for 4.2 percent of the population, they represented 17 percent of the workers in service positions. Most notably, 55 percent of the male barbers and hairdressers and 97 percent of the bootblacks were Italian. Italians were also slightly overrepresented in manufacturing and mechanical pursuits; they accounted for 5.5 percent of the total, including 34 percent of the shoemakers and 18 percent of the masons. And 9 percent of the retail merchants and 16 percent of the peddlers were Italian.¹¹ Thus, although Italians in New York were numerous in a few occupations, they never dominated any sector of the economy as Italians did in industry and to a lesser extent commerce in Buenos Aires.

The relative distribution of Italians in white- and blue-collar occupations also differed in the two cities. Both in 1880 and in 1905, approximately three-quarters of the Italians in New York were blue-collar workers, most of whom were in unskilled or semi-skilled construction, transportation, factory, or domestic service jobs. White-collar peddlers, shopkeepers, and barbers accounted for most of the remaining one-quarter. High white-collar occupations provided only 2 percent of the total. The only significant group of professionals were musicians and teachers of music.¹² Most Italians in Buenos Aires also held blue-collar jobs, but a substantially higher proportion (30 to 35 percent in Buenos Aires versus 13 to 22 percent in New York) were skilled workers. Although approximately one-quarter entered white-collar occupations, as was the case in New York, the distribution differed; many more Italians in Buenos Aires were owners of small industrial and commercial establishments as opposed to peddlers and barbers. Furthermore, although the percentage of high white-collar jobs was small in Buenos Aires, it was twice as large as that in New York (4 percent compared to 2 percent); there were significant numbers of Italians in Buenos Aires among the health, education, and fine arts professionals and among engineers.¹³

¹⁰ Manual arts are included in industry. Some of the figures for Argentina are estimates based on Buenos Aires, *Censo general de la Ciudad de Buenos Aires, 1887*, 2 (Buenos Aires, 1889): 36, 306, 379, and *Censo general de la Ciudad de Buenos Aires, 1909*, 1 (Buenos Aires, 1910): 132, 135, 150, 155.

¹¹ U.S. Senate, 61st Congress, 3d Session, *Reports of the Immigration Commission* [hereafter, U.S. Immigration Commission, *Reports*], 41 vols. (Washington, 1911), 28: 168–76; and U.S. Bureau of the Census, *Special Reports: Occupations at the Twelfth Census* (Washington, 1904), 634–38.

¹² U.S. Immigration Commission, *Reports*, 23: 168–76; U.S. Bureau of the Census, *Special Reports: Occupations*, 634–38; and Kessner, *The Golden Door*, 52.

¹³ These figures are estimates based on the data in República Argentina, *Segundo Censo nacional de la República Argentina, 1895*, 2 (Buenos Aires, 1898): 47–50, and *Tercer Censo nacional de la República Argentina, 1914*, 10 vols. (Buenos Aires, 1916), 4: 201–12. Also see Herbert S. Klein, "La Integración de inmigrantes italianos en la Argentina y los Estados Unidos: Un Análisis comparativo," *Desarrollo Económico* (Buenos Aires), 21 (1981): 3–27. Another important set of figures to compare is relative real wages and buying power. Some information exists for both Argentina and the United States, but the comparison is hazardous, and I have not yet worked



Figure 1: Bank ("Banca Italiana") and Bar of Liccione, Pittaro, and Co., corner of Hester and Mulberry Streets, New York, 1893. (The bank's earlier headquarters are shown on the front cover.) Pittaro is second from left and G. Liccione is second from right. When this photograph was taken, the new proprietors had not yet taken down the sign above the store from the previous owner, Henry Elias, a brewer. University of Minnesota, Twin Cities, Immigration History Research Center Collection, Vito Pittaro Papers.

The Italian immigrant groups in both cities experienced some upward occupational mobility but of a different sort. Although there was a substantial increase in semi-skilled (7 to 16 percent) and skilled (13 to 22 percent) Italian workers in New York between 1880 and 1905, there was a decline among white-collar workers (25 to 18 percent). In Buenos Aires, the percentage of both skilled and white-collar Italian workers increased slightly between 1895 and 1914. And a few immigrants were able to move into the leadership ranks of the army, the church, and politics. Thus, there was some upward mobility among Italians in New York, but it was confined to blue-collar workers; in Buenos Aires, however, mobility—however slight—extended to white-collar occupations as well.¹⁴

Italians in Buenos Aires also played a more significant role in local workers' and

out a satisfactory analysis. See Roberto Cortes Conde, *El Progreso Argentino, 1880–1914* (Buenos Aires, 1979), 240–69; Paul H. Douglas, *Real Wages in the United States, 1890–1926* (New York, 1966); Clarence D. Long, *Wages and Earnings in the United States, 1860–1890* (Princeton, 1960); and Albert Rees, *Real Wages in Manufacturing, 1890–1914* (Princeton, 1961).

¹⁴ The Buenos Aires figures are estimates based on República Argentina, *Segundo Censo nacional de ... 1895*, 47–50, and *Tercer Censo nacional de ... 1914*, 4: 201–12. Also see Kessner, *The Golden Door*, 52; U.S. Bureau of the Census, *Special Reports: Occupations*, 634–38; José Luis de Imaz, *Los Que Mandan* (Albany, N.Y., 1970); Dario Canton, *El Parlamento Argentino en épocas de cambio* (Buenos Aires, 1965); and Robert A. Potash, *The Army and Politics in Argentina* (Stanford, 1969). Many second-generation Italians moved rapidly into positions of leadership in Argentine society. One of them, Carlos Pellegrini, was president of the country from 1890 to 1892.

employers' organizations than did those in New York. Organized labor in Buenos Aires was almost completely the product of the various immigrant groups. In the 1880s and 1890s the immigrants attempted several times to organize national confederations, and by the first decade of the twentieth century they had succeeded in establishing two rival organizations. Italians made up about 40 percent of the organized workers, provided the most important leadership within the movement, and were much more active in labor organizations and in strikes than were native Argentines. In New York, the Italians were slow to organize. The Immigration Commission concluded that southern Italians were less organized than native workers or foreigners in general. Although by 1914 Italian stevedores, garment workers, bricklayers, and barbers had achieved some success at organizing, they never played a leadership role in New York or on the national level.¹⁵

The same pattern held true for employers' organizations. Italians were influential in the development and growth of the Argentine Industrial Union. From its founding in 1887, at least 20 percent of the administrative council and one of its officers was a first- or second-generation Italian, and an Italian was president of the Industrial Union in twenty-one of the thirty-one years between 1905 and 1935. In New York, Italians played no role in the development of the National Association of Manufacturers, and not one of the presidents or members of the executive committee was a first- or second-generation Italian during the first twenty years of the organization's existence (1895–1914).¹⁶

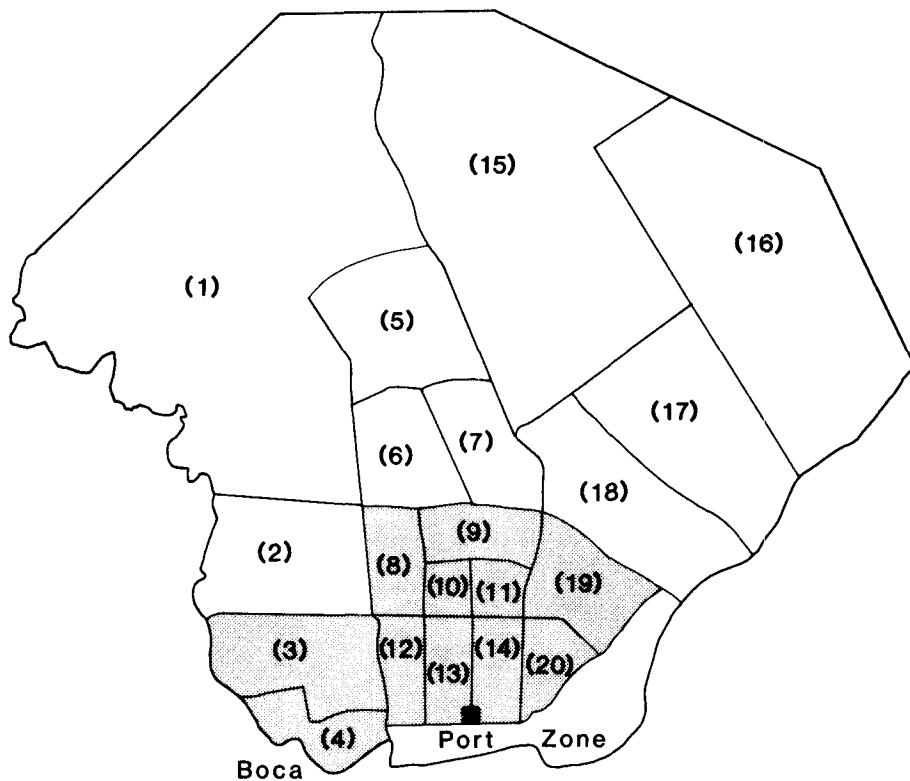
The patterns of Italian participation in economic life in Buenos Aires and New York were strikingly different. In Buenos Aires, Italians—who accounted for a much larger percentage of the total population—naturally represented a higher percentage of the workers. Yet they were more effective than those in New York in areas where numbers alone did not make the difference. They formed a higher percentage of the skilled and white-collar workers than their relative proportion of the population, as a group experienced greater upward mobility especially among skilled and white-collar workers, were more active than the native Argentines in the economy, and were a major force in the development of workers' and employers' organizations. Italians in New York, in contrast, were less successful than their counterparts in Buenos Aires at obtaining jobs in the upper levels of the occupational structure and at organizing to protect their economic interests.

Housing was another major concern of the Italian immigrants to both cities. Most immigrants started out in the crowded, low-rent districts in the center of the metropolitan area, where living conditions were poor. To improve their situation, many Italians moved to homes they could buy and to residences outside the center

¹⁵ Baily, "Italians and Organized Labor" (1967), 55–66; U.S. Immigration Commission, *Reports*, 7: 417–19, and 23: 368–75; Pozzetta, "The Italians of New York City, 1890–1914," 337–64; and Fenton, *Immigrants and Unions—A Case Study*, 574.

¹⁶ Eugene G. Sharkey, "Union Industrial Argentina, 1887–1920: Problems of Industrial Development" (Ph.D. dissertation, Rutgers University, 1978), 46–89. Professor Sharkey very kindly shared with me data on the nationality of the UIA leadership, which is not included in his dissertation. Also see Albert K. Steigerwalt, *The National Association of Manufacturers, 1895–1914: A Study in Business Leadership* (Ann Arbor, Mich., 1964), 175–83.

Map 1: Buenos Aires Census Districts

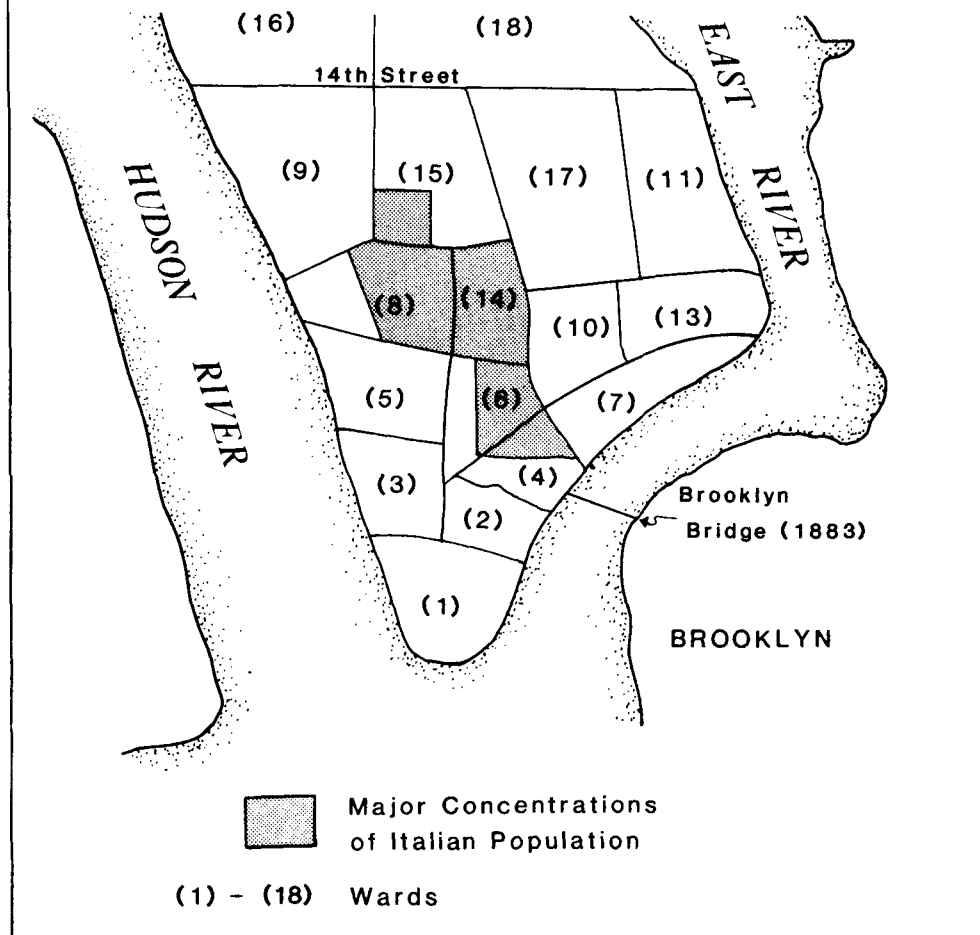


Location of the Eleven Central Districts

(1) - (20) Census Districts

city. Italians in Buenos Aires were more dispersed throughout the city in 1887 and in 1904 than those in New York. In 1904, they—like the rest of the population—lived predominantly in the eleven census districts located within a three-mile radius of the downtown business center, and they accounted for at least 14 percent of the total population in every district (see Map 1). They did, nevertheless, cluster somewhat within this area; while 41 percent lived in the five districts on the perimeter of the downtown business center (3, 4, 8, 9, and 19), only 29 percent resided in the six most central districts (12, 13, 14, 20, 10, and 11). Italians in New York, however, concentrated in only a few areas. By the 1870s, a substantial number lived in the sixth and fourteenth wards of Manhattan, and they gradually spread out into several neighboring wards and into Harlem, in the area between Second Avenue and 110th Street (see Map 2). In 1890, 64 percent lived in the Bowery colony and another 10 percent in Harlem. Gradually they moved to other

Map 2: Lower Manhattan Wards and Italian Districts



areas of the city, but before World War I they for the most part remained in these areas of Manhattan.¹⁷

The living conditions of the Italians in both cities were generally poor. Most suffered from the effects of overcrowding, inadequate sanitary facilities, and unsatisfactory health conditions. A description of New York immigrant housing by the Tenement House Department could equally well have been applied to Buenos Aires.

Many cases have been found where plumbing fixtures have been removed and the pipes left open. . . . [T]he tenants have used the dumbwaiter shaft as a chute for the disposal of

¹⁷ Buenos Aires, *Censo general . . . , 1887*, 33, 65, and *Censo general de la Ciudad de Buenos Aires, 1904* (Buenos Aires, 1906), 6, 84; República Argentina, *Tercer Censo nacional de . . . 1914*, 2: 129–48; Kessner, *The Golden Door*, 127–60; U.S. Industrial Commission, *Reports*, 15 (Washington, 1902): 471–74; and U.S. Bureau of the Census, *Vital Statistics of New York and Brooklyn, 1885–1900* (Washington, 1901), 230–37.

rubbish, fecal matter and garbage. . . . [T]he water closets have been stopped up for weeks. . . . Often the roofs are not repaired, and after a storm the water soaks through the plastered ceilings. . . . Bedrooms are often festooned with cobwebs hanging from the ceiling.¹⁸

Yet the Italians in Buenos Aires were somewhat better off. In the Argentine capital, less than one-quarter lived in crowded tenement houses, compared to more than three-quarters in New York. In addition, the population density per acre of the Italian districts in New York was approximately 50 percent higher than it was in Buenos Aires. The remaining, nontenement-house working-class living quarters in Buenos Aires may not have been much better, but at least they were less crowded.¹⁹

Property ownership was also higher among the Italians in Buenos Aires. In 1887 and in 1904 approximately 12 percent owned their own homes, and by 1914 this figure had risen to 17.6 percent. Even in the areas of heaviest concentration, such as the Boca (district 4), the percentage of Italian homeowners was substantial: 8 percent in 1904 and 11 percent in 1914. Data on property ownership in New York are fragmentary but suggestive. The Immigration Commission's survey of 524 households in 1909 indicates that only 1.3 percent of the Italian immigrants owned their homes. Even if that figure were doubled or tripled, to account for the possible underrepresentation of property owners in the sample, it would still be far lower than the percentage for Buenos Aires. Clearly, in the years before World War I, New York Italians were much less successful in achieving the goal of property ownership than were those in Buenos Aires.²⁰

Although Italians in both cities changed their residences frequently, they may not have improved their living conditions by doing so. Many immigrants left New York and Buenos Aires for places in other parts of their adopted country, and others relocated within their own neighborhood. We cannot trace those who left either city, nor can we make any meaningful judgments about the significance of intraneighborhood moves. We can, however, measure the movement to the outer areas of the two cities, where conditions were less congested and the opportunity to purchase a home was greater.²¹ In Buenos Aires, Italians at first resided almost completely within the central area; as of 1904, 70 percent still remained there (districts 3, 4, 8, 9, 10, 12, 13, 14, 19, and 20). By 1914, however, only 47 percent lived in the center, and 53 percent lived in the outlying districts. Although Italians in New York first settled in Manhattan and gradually spread out to Brooklyn and the other boroughs of the city, they moved from the center at a later date than

¹⁸ City of New York, Tenement House Department, *First Report*, 1 (New York, 1903): 6–7; Luigi Villari, *Gli Stati Uniti d'America e l'emigrazione italiana* (New York, 1975), 212–22; and James R. Scobie, *Buenos Aires: Plaza to Suburb, 1870–1910* (New York, 1974), 146–59.

¹⁹ Kessner, *The Golden Door*, 132; Buenos Aires, *Censo general . . . , 1904*, 84; and Charles S. Sargent, *The Spatial Evolution of Greater Buenos Aires, Argentina, 1870–1930* (Tempe, Ariz., 1974), 150.

²⁰ The data of the Immigration Commission are limited to heads of Italian households; they do not apply to the Italian population as a whole. Thus, the percentage of all Italians who owned their own homes in New York may have been as low as or lower than the 1.3 percent figure used in the sample. See U.S. Immigration Commission, *Reports*, 26: 209; Kessner, *The Golden Door*, 150–52; and Buenos Aires, *Censo general . . . , 1887*, 104, and *Censo general . . . , 1904*, 84.

²¹ U.S. Immigration Commission, *Reports*, 26: 244; Kessner, *The Golden Door*, 142; and Società Italiana Unione e Benevolenza, "Elenco dei soci, 1895–1902," Archiv d'Unione e Benevolenza, Buenos Aires.

those in Buenos Aires. In 1900, 66 percent lived in Manhattan, and ten years later 59 percent still resided there. Only after World War I did more than half of New York's Italians live outside of Manhattan.²²

Several factors influenced patterns of settlement and dispersion. Italians, especially those who worked long hours for low pay, lived near their places of employment. Italians in New York, therefore, lived below 14th Street in Manhattan, where, as late as 1906, 67 percent of all factory jobs were located. In Buenos Aires, industry and commerce were concentrated in the central area, yet, because the city was not an island, the Italians could spread out and still be close to their jobs. As industry and commerce expanded to other parts of each city, the Italians followed.²³ Although the transportation system influenced where an immigrant might move when he decided to relocate, it did not necessarily determine the timing of his move. Elevated trains began to run from the Battery to Harlem in 1881, which clearly facilitated the development of the Harlem colony. By the early 1900s, however, the subway system connected Brooklyn, Queens, and the Bronx with Manhattan, yet the major outward movement in New York did not occur until after World War I. Although in Buenos Aires the rail system linked various parts of the city, not until the introduction of the electric streetcar and the drastic reduction of fares in the early 1900s did Italians in large numbers begin to move to the outlying districts.²⁴

Old World ties also proved important in determining patterns of settlement. Italians in both cities frequently settled and then moved in village- and language-based clusters.²⁵ Migrants from Agnone to Buenos Aires, for example, lived in District 14, especially in two nearby clusters on Calle Córdoba. Those from Bagnara also lived in District 14, but in a four-block area between Calles Reconquista and 25 de Mayo. Amedeo Canuti and other Sirolesi lived in three clusters within the Boca (District 4). A second colony of Sirolesi developed in Quilmes, about eleven miles and several train stops away from the Boca. Amedeo and one of his brothers moved to Quilmes, but the other brothers remained in the Boca.²⁶

Similarly, Italians in New York generally settled by villages. In 1890 the Neapolitans populated the area of Mulberry Bend, the Genovesi Baxter Street near the Five Points area, and the Sicilians, like Giuseppe Trimora, Elizabeth Street between Houston and Spring Streets. In 1920, more than two hundred families from Cinisi, Sicily, lived in the area of 6th Street and Avenue A. Other clusters from

²² Buenos Aires, *Censo general . . . , 1904*, 6, 84; República Argentina, *Tercer Censo nacional de . . . 1914*, 2: 129–49; and U.S. Bureau of the Census, *Thirteenth Census of the United States . . . , 1910*, 1 (Washington, 1913): 824, 827–28, and *Fourteenth Census of the United States . . . , 1920*, 2 (Washington, 1922): 47, 747–49.

²³ Kessner, *The Golden Door*, 150; Scobie, *Buenos Aires: Plaza to Suburb*, 70–113, 135–46; and Sargent, *Greater Buenos Aires, Argentina*, 22–25.

²⁴ Kessner, *The Golden Door*, 148–49; Scobie, *Buenos Aires: Plaza to Suburb*, 160–80; and Sargent, *Greater Buenos Aires, Argentina*, 66–76.

²⁵ These clusters are normally referred to as "chains." The term "chain migration" is subject to differing interpretations, yet most agree on the essential point: informal personal networks of individuals, kinship groups, villagers, and language clusters in the sending and receiving areas were a major influence on how most individual Italians chose their destinations, how they got there, where they settled, how they got jobs, and with whom they associated. For a fuller discussion of this idea, see Bailly, "Chain Migration of Italians to Argentina," 73–75.

²⁶ Bailly, "Chain Migration of Italians to Argentina," 73–91.

Cinisi lived in the Bowery, Harlem, and Brooklyn.²⁷ Presumably, the relationship between the Cinisi colonies in Manhattan and Brooklyn was similar to that of the Sirolo colonies in the Boca and Quilmes.

Although the availability of jobs, inexpensive transportation, and village chains primarily determined patterns of residence and movement, they do not by themselves explain the earlier movement of Italians to outlying areas of Buenos Aires. Circumstantial evidence indicates that the greater ability of the immigrants in Buenos Aires to save does more to account for the early relocation of Italians in the outlying districts of the city. Because more Italians in Buenos Aires held more prestigious, better-paying jobs, they were presumably in a better position to save. Since the main purpose of the move outward was often to buy a house with some land, relocation was postponed until accumulated savings permitted such a purchase. In New York most Italian immigrants were unable to accumulate the necessary savings until after World War I; thus, the move outward came later.²⁸

Italians in Buenos Aires were more dispersed, lived in somewhat better physical circumstances, were more successful in acquiring property, and were residentially upwardly mobile at an earlier date than those in New York. This difference must not be overstated. Most Italians in both cities lived in very poor circumstances. Nevertheless, there was a difference. And that difference meant that Italians in Buenos Aires were more likely to have found better housing.

Immigrant institutions often played an important role in the adjustment of the Italians to their new environment. Both in New York and Buenos Aires, the Italians established mutual aid societies, social and recreational clubs, hospitals, banks, chambers of commerce, churches, and newspapers to help them meet their needs.²⁹ To the extent that these organizations involved the immigrants, had the necessary resources, and developed representative leadership, they could provide social and economic services and a structure that served to integrate and strengthen the community. The stronger and more united the community, the better able it was to defend the interests of its members within the new society.

Mutual aid societies were the most important of these immigrant institutions; more Italians in both New York and Buenos Aires joined these societies than any other kind of organization. Some societies recruited members exclusively from a specific village or area of Italy. Others were more broadly based, bringing together individuals from all parts of Italy. Still others were formed by workers in the same occupation. All of them sought to provide essential death, sickness, and unemployment benefits for their members as well as a congenial social environment.

Both the New York and Buenos Aires mutualist movements began in the mid-

²⁷ U.S. Industrial Commission, *Reports*, 474; Pozzetta, "The Italians of New York City, 1890-1914," 995-97; John Horace Mariano, *The Italian Contribution to American Democracy* (New York, 1975), 19-22; and Robert E. Park and Herbert A. Miller, *Old World Traits Transplanted* (New York, 1921), 146-58.

²⁸ Kessner, *The Golden Door*, 151; and Robert E. Shipley, "On the Outside Looking In: A Social History of the *Porteña* Worker during the 'Golden Age' of Argentine Development, 1914-1930" (Ph.D. dissertation, Rutgers University, 1977), 173-96. The availability of credit was another factor that influenced the timing of the move outward, but I have not been able to gather sufficient data to make a meaningful comparison of the two cities. See Scobie, *Buenos Aires: Plaza to Suburb*, 182-91; and Anton F. Mannel, "New York City Population Trends Related to Mortgage Lending" (Ph.D. dissertation, Rutgers University, 1941), 86-117.

²⁹ Pozzetta, "The Italians of New York City, 1890-1914," 231-304; and Zuccarini, *I Lavori degli Italiani*.

nineteenth century and expanded with each new wave of immigrants. Although many of the largest and most important societies in Buenos Aires were established before 1880, more were founded during the last two decades of the nineteenth century than at any other time. In New York, growth was very gradual, and by far the greatest number of them were established in the decade or so prior to World War I. By 1910 in Buenos Aires there were seventy-five mutual aid societies with approximately fifty-two thousand members out of an Italian population of some two hundred and eighty thousand. In New York at about the same time there were as many as two thousand societies with an estimated total membership of forty thousand out of an Italian population of three hundred and forty thousand.³⁰

The two movements differed in ways that significantly affected their ability to serve their Italian communities. Divisiveness, based on personal rivalries and Old World ties, typified the mutual aid movement in New York. The overwhelming majority of the societies were small (under five hundred members), poorly financed and managed, and restricted to immigrants from the same town or region of Italy. Although a few societies opened their membership to all Italians, these few never attained the size, wealth, or influence necessary to establish effective leadership of the mutualist movement. Most of the societies were made up of artisans, but the top officers frequently were successful businessmen. Louis V. Fugazy, a noted banker and labor agent, was the prime mover in more than one hundred mutual aid societies and was president of fifty. Beginning in 1905, many societies joined the National Order of the Sons of Italy, and that organization thus gradually gained influence within the community. But the Sons of Italy was really a loose confederation of societies, and it too lacked the resources to be of significant help to most immigrants.³¹

In Buenos Aires, however, a number of large societies (one thousand or more members) dominated the movement and provided a distinct kind of leadership. These societies had substantial assets in both buildings and capital reserves. They also performed more extensive services for their members; in addition to the normal insurance and social benefits, they provided schools, medical clinics, hospital care, pharmacies, restaurants, and, in some cases, job placement services. Although skilled artisans formed the largest subgroup of members in most Argentine societies, these organizations also included substantial numbers of semi-skilled and white-collar workers. The leaders came almost exclusively from the white-collar members. Furthermore, Italians from all parts of the peninsula joined these societies and rose to positions of leadership within them. Although local ties, politics, and personal rivalries led to some divisions, the large societies were able to develop a relatively united mutualist movement by World War I.³² The wealthier,

³⁰ Figures on the number of mutual aid societies and on their membership vary enormously for New York. Edwin Fenton's figures seem the most reasonable to me; *Immigrants and Unions—A Case Study*, 50. Also see "L'Elemento italiano rappresentato nelle associazioni di New York, Brooklyn, Hoboken, e Newark, 1884," Archive of the Center for Migration, New York; "Le Società italiane all'estero nel 1908," *Bollettino dell'Emigrazione*, 24 (1908): 1–147; "Le Società italiane negli Stati Uniti dell'America del Nord," *ibid.*, 4 (1912): 19–54; and Baily, "Las Sociedades de ayuda mutua," 491.

³¹ Pozzetta, "The Italians of New York City, 1890–1914," 244–65; and Fenton, *Immigrants and Unions—A Case Study*, 49–54.

³² Baily, "Las Sociedades de ayuda mutua," 487–94.

better administered, and more unified mutualist movement in Buenos Aires provided more extensive services for a larger percentage of the Italian population than did the movement in New York.

The Italian-language newspaper was probably the second most important immigrant institution in Buenos Aires and New York. No one joined newspapers, but more Italians read them than there were members of mutual aid societies. Tens of thousands of immigrants scanned their columns in search of job opportunities, notices on the activities of community organizations, tips on how to survive and prosper, guidance in understanding and adjusting to the wider community in which they lived, and information on Italy. Since these papers reached more Italians than any other immigrant organization, they were in the best position to unite the community.

In Buenos Aires, the first Italian newspaper was established in the 1860s. Dozens of others were published in subsequent years, but most had small circulation and did not last very long. *La Patria degli Italiani* had by far the largest circulation and continued publishing longer than any other paper. Founded in 1876 by Italian journalist Basilio Cittadini, it reached a circulation of approximately sixty thousand—or 19 percent of the Italian community—in 1914. *La Patria* was a liberal, anticlerical, and moderately antimonarchist morning daily that attempted to serve the interests of immigrants of all social and income levels within the Italian colony in Buenos Aires. It regularly published information on working and living conditions and explained the grievances of workers involved in labor disputes. Every issue included letters from immigrants, who most frequently described their problems and asked for help. The editors of *La Patria* interpreted Argentine society for the Italian immigrants, defended their various interests, arbitrated some of their disputes, and often spoke for them as a community. In addition, the paper performed several, more unusual functions, particularly in the decade prior to World War I: it ran free medical, legal, and agricultural clinics for its subscribers. These services gave *La Patria* something of the character of a mutual aid society and increased its impact on the community.³³

The first Italian-language newspaper in New York began publication in 1849, and many others of short duration and small circulation appeared in the following half-century. The largest and most long-lived of these was *Il Progresso Italo-Americano*. Established in 1879 by businessman and labor contractor Carlo Barsotti, the paper reached a circulation of roughly eighty thousand—or 22 percent of the Italian community—by the beginning of World War I. *Il Progresso*, however, made little effort to serve the interests of all of the Italians in New York. It was, most significantly, antilabor, probably because Barsotti was afraid that labor unions might undermine his other businesses. Barsotti frequently used the paper as a vehicle to further his personal interests. “Publishers,” as one author explained, “used their journals to advertise their own far-flung enterprises, to praise themselves and their friends, to promote projects in which they were interested, and to

³³ Baily, “The Role of the Press and the Assimilation of Italians in Buenos Aires,” 321–40.

attack anyone with whom they disagreed.”³⁴ Although *Il Progresso* provided information on the happenings in the Italian community, defended all Italians against attacks by outsiders, and perhaps helped break down village-based provincialism and foster a larger group awareness, it did little to interpret American society to the immigrants or to strengthen the community as a whole. *Il Progresso* supported the needs of some of the Italians in New York, but it opposed that of the large majority of workers.³⁵ *La Patria* was more effective in serving the Italian immigrants in Buenos Aires than *Il Progresso* was in New York.

The supporting evidence for some parts of this analysis of the adjustment process of Italians in Buenos Aires and New York is not as complete as it is for others. The evidence on housing conditions, home ownership, residential mobility, economic activity, and community organizations does, nevertheless, all point in the same direction. And, although future studies will certainly provide more information on the Italian immigration experience in both cities, they are not likely to alter substantially the basic conclusion. Italians in Buenos Aires adjusted more rapidly and successfully to their new environment than did those in New York.

THE EARLIER AND GREATER SUCCESS of Italians of Buenos Aires in adjusting to their new environment is clear. The reasons for the discrepancy are somewhat more complex. Were the Italians in Buenos Aires in some way better prepared for the immigration experience than those who went to New York? Did they encounter a more receptive host society? What, if anything, did they do to improve their chances of success? Although a considerable number of interrelated variables explain the different experiences of the Italians in the two cities, they can be divided into three categories: those relating to the character of the immigrant groups when they migrated, to the kind of societies they found, and to the changing nature of the immigrant community over time.

When Italians migrated to New York and Buenos Aires, they brought with them certain attributes that influenced their ability to find jobs and housing and to organize community groups. Among the most important were not only occupational and organizational skills but also expectations regarding the permanency of the move. Unskilled laborers predominated in both groups: 75 percent of those who went to the United States between 1899 and 1910 and perhaps 60 percent of those who went to Argentina were unskilled farm or common workers. Only 25 percent of those who migrated to the United States and 40 percent of those who migrated to Argentina were listed as skilled or white-collar workers. The statistics do not break down the overall figures for those who went specifically to Buenos Aires and New York; but these centers of economic activity undoubtedly attracted at least the same proportions of skilled and white-collar workers, and most likely an even higher percentage of them than the national norm for each country. Italians in

³⁴ Fenton, *Immigrants and Unions—A Case Study*, 59.

³⁵ Pozzetta, “The Italians of New York City, 1890–1914,” 234–42; Fenton, *Immigrants and Unions—A Case Study*, 58–63; and Robert E. Park, *The Immigrant Press and Its Control* (New York, 1922), 449.

Buenos Aires as a group were also more literate than those in New York; some 60 percent were literate in Buenos Aires compared to about 50 percent in New York.³⁶

A larger proportion of the Italians who migrated to Buenos Aires were from northern Italy. They were in general not only more skilled and literate than Italians from the south but also more familiar with organizations such as labor unions and mutual aid societies. While 42 percent of those who went to Argentina came from the north, 46 percent from the south, and 12 percent from the center, 80 percent of those who went to the United States were southerners.³⁷ And not all of their skills were transferable to their new environments. Skills appropriate to rural occupations such as farming were of little use in urban New York and Buenos Aires; those of artisans and white-collar workers for the most part were. With better occupational skills, higher rates of literacy, and greater familiarity with organizations, the Italians in Buenos Aires had an advantage in finding higher status jobs and in developing community institutions to protect their interests.

Expectations regarding the permanency of their emigration differed to some extent between those going to the northern and those to the southern hemisphere. Measuring the Italians' intentions when they left their villages and towns for the New World is, of course, impossible. Nevertheless, data on their respective rates of return and on sex and age composition of the migrants are at least indicative of intent. The percentage of Italians who repatriated for the period 1861–1914 is slightly lower for the Argentine immigrants than it is for the U.S. immigrants (47 percent compared to 52 percent). Those who returned to Italy from both countries were overwhelmingly unskilled males, and New York had a larger proportion of unskilled Italian males than did Buenos Aires. Approximately 30 percent of the immigrants repatriated from Buenos Aires during the decade and a half before World War I, whereas approximately 50 percent repatriated from New York during those same years.³⁸

³⁶ The literacy figures for Buenos Aires refer to all Italians in the city eight years old or more; the U.S. Immigration Commission figures refer to Italians fourteen years old or more entering the United States or to Italian male heads of households in New York. Thus, the actual difference in literacy was probably greater than I have suggested. See Kessner, *The Golden Door*, 33–36; U.S. Immigration Commission, *Reports*, 3: 84, 95–96, 131–38, and 26: 238; Juan A. Alsina, *La Inmigración en el primer siglo de la independencia* (Buenos Aires, 1910), 92–93; Mario Nascimbene, "Inmigración y analfabetismo en Argentina: La Corriente Migratoria italiana entre 1876 y 1925," *Sociológica*, 1 (1978): 121–70; Buenos Aires, *Censo general . . . , 1887*, 65; and República Argentina, *Tercer Censo nacional de . . . 1914*, 3: 321.

³⁷ The pre-1919 regions of Italy were as follows: the north included Piemonte, Lombardia, Trentino, Veneto, Emilia, and Liguria; the center included Toscana, Umbria, Marche, and Lazio; and the south included Abruzzi, Puglia, Basilicata, Calabria, Campania, Sicilia, and Sardegna. For a detailed breakdown of the regional origins of the immigrants, see Commissariato Generale dell'Emigrazione, *Annuario Statistico della emigrazione italiana dal 1876 al 1925* (Rome, 1926), 145–51. On organizations in Italy, see Barton, *Peasants and Strangers*, 64–67; and Briggs, *An Italian Passage*, 15–36.

³⁸ The *golondrinas*—the Italians who went every year to Argentina to harvest the crops—numbered about thirty thousand annually during the decade or so prior to World War I. Because they did not go to Buenos Aires, I have subtracted this number from the total number of returnees to arrive at my estimate. The same cannot, however, be done for the New York Italians. As many temporary migrants went to New York as to other parts of the United States; they apparently remained in the United States for a somewhat longer period than did the temporary migrants to Buenos Aires. Thus, I am using the same figure for New York as for the United States as a whole. See Gould, "European Inter-Continental Emigration—The Road Home," 41–71; Caroli, *Italian Repatriation from the United States*, 25–50; Mark Jefferson, *Peopling the Argentine Pampa* (New York, 1926), 183; Commissariato Generale dell'Emigrazione, *Annuario Statistico*, 830–913; and República Argentina, Dirección de Inmigración, *Resumen estadística del movimiento en la República Argentina, 1857–1924* (Buenos Aires, 1925), 4–5.

Demographic distribution of the Italian immigrants in the two cities according to age and sex provides supporting evidence for the relatively greater permanency of the Italian settlement in Buenos Aires. Italians who migrated to both cities were predominantly males between fourteen and forty-four years old. In Buenos Aires, males represented a little less than two-thirds of the Italian population in 1895 (61 percent) and 1914 (62 percent), and two-thirds of all immigrants (65 percent) were between the ages of fourteen and forty-four. The percentage of Italian males and of fourteen- to forty-four-year-old Italian immigrants was higher in New York. During the three decades before 1910 approximately 77 percent of the Italian immigrants to the United States were males, and nearly 80 percent of these were between fourteen and forty-four years of age. There is no reason to assume that the percentages for New York City were any lower. Therefore, while roughly two out of every three Italians who went to Buenos Aires were males between the ages of fourteen and forty-four, approximately four out of every five Italian immigrants in New York were in these age and sex categories.³⁹

These figures, significantly, indicate a higher percentage of women, children, and older people in the Italian population of Buenos Aires. This in turn is suggestive of a larger proportion of family units and probably greater permanence for the immigrant community in Buenos Aires. It is, of course, possible that more Italians got married after they arrived in Argentina than they did in the United States, but it is more likely that a greater number of families migrated to the Argentine capital in the first place. To the degree that rates of return and sex and age composition reflect intentions, it seems that more Italians who migrated to Buenos Aires expected to remain and that more who came to the United States were temporary migrants who planned, when they left Italy, to return.

Although the Italians who arrived in New York and Buenos Aires during the forty-four years under consideration here shared to some extent a similar general culture based on a common language, religion, and set of values, they did not have identical group characteristics. Those who migrated to Buenos Aires included more workers with higher levels of skill and of literacy, more individuals with experience in organization, and more people who intended to stay. These differences gave the Italians in Buenos Aires an initial advantage as they sought to find jobs and to develop community institutions to protect their interests. But these differences do not alone account for the vast difference in the two Italian colonies. The receiving environments the immigrants entered also varied dramatically and significantly affected the immigrants' adjustment. The economy, the host society's perception of the Italians, the culture, and the existing foreign-born community in Buenos Aires contrasted sharply with those in New York.

The economies of the two urban societies were strikingly different. In 1869, at the time of the first national census, Argentina was sparsely populated and economically far less developed than Western Europe or the United States. Those

³⁹ República Argentina, *Segundo Censo nacional de ... 1885*, 15, 28–29, and *Tercer Censo nacional de ... 1914*, 3: 12–13; Massimo Livi-Bacci, *L'immigrazione e l'assimilazione degli Italiani negli Stati Uniti* (Milan, 1961), 15; U.S. Immigration Commission, *Reports*, 26: 176–77; and Commissariato Generale dell'Emigrazione, *Annuario Statistico*, 187–200.

who ruled Argentina during the next forty years were determined to replace the stagnant economic and social structures inherited from Spanish colonial rule with the most dynamic and advanced systems to be found anywhere. They sought to institute such sweeping change by encouraging massive immigration, improving the educational system, introducing new technology, modernizing agriculture and livestock breeding, developing a transportation infrastructure (notably railroads and port facilities), and wooing foreign investment capital. By 1914, the export-oriented, landed elite had succeeded in accomplishing most of its objectives. Argentina had, in a relatively short time, become one of the leading agricultural and livestock-breeding countries in the world. It was a major exporter of grains, the leading supplier of beef to England, and a center of industries to process agricultural and pastoral products for export.⁴⁰

The rapidity of Buenos Aires's economic development, which coincided with the wave of Italian migration, provided opportunities at all levels for the new residents. When Argentine development began, the country had no vital middle class, no significant skilled working class, no other numerically important immigrant groups, and no labor movement or employers' organizations. The traditional native elite continued to confine itself primarily to land and politics, which left commerce, industry, and some of the professions and economic organizations to the immigrants, especially to the large Italian community.

The United States had a much larger population and a more highly developed economy in 1870 than did Argentina. During the next forty-four years, the United States expanded both its heavy and light industry, and by World War I had become a major industrial as well as agricultural and commercial country. The Italians who migrated to New York encountered thriving professional, middle, and skilled working classes composed of native-born Americans and previous immigrants. What the New York economy needed was unskilled construction and industrial workers. There were opportunities for Italians, but opportunities of a kind different from those in Buenos Aires. As a result, the Italians entered the economy at the lower levels and had greater difficulty in achieving upward occupational mobility. More restricted economic opportunity in New York limited the growth of upper-level Italian occupational groups, the resources of the Italian community, and the development of immigrant institutions—all of which made adjustment to the new environment more formidable.⁴¹

The host society's perceptions of the Italians also influenced adjustment. The Argentine elite viewed the immigrants as a means of "civilizing" the country as well as developing its economy. These leaders believed that the native population of the

⁴⁰ James R. Scobie, *Argentina: A City and A Nation* (New York, 1971), 64–135; and Germani, "Mass Immigration and Modernization in Argentina," 289–330.

⁴¹ Klein, "La Integración de inmigrantes," 3–27; Peter S. Shergold, "Relative Skill and Income Levels of Native and Foreign-Born Workers: A Re-Examination," *Explorations in Economic History*, 13 (1976): 451–61; Paul F. McGouldrick and Michael B. Tannen, "Did American Manufacturers Discriminate against Immigrants before 1914?" *Journal of Economic History*, 37 (1977): 723–46; and Gordon W. Kirk, Jr., and Carolyn Tyirin Kirk, "The Immigrant, Economic Opportunity, and Type of Settlement in Nineteenth-Century America," *ibid.*, 38 (1978): 226–34.

interior—symbolized by the *gaucho* (“cowboy”) and the *caudillo* (“charismatic leader”)—was backward and inferior. European immigrants would, they thought, intermix with the indigenous population and in time help create a biologically superior population. The tiny Argentine elite may have looked down upon Italian immigrants as social inferiors but still saw them as superior to 95 percent of the native population. In this sense, the Italians were bearers of civilization. By the early 1900s, some members of the elite did become concerned about the dominating influence of the immigrants in Argentina. This concern, however, remained limited as long as the economy continued to grow, and it did not translate into significant restrictions either on immigration or on the opportunities for the immigrants until the Depression of the 1930s.⁴²

In contrast, Italians who went to New York were viewed in an entirely different way. Many New Yorkers viewed the immigrants—especially the great mass of southern Italians—as an inferior race that threatened to dilute the good Northern European stock and undermine traditional American institutions. The Italians provided the unskilled labor necessary for the growth of the economy, but the native-born elite certainly did not see them as bearers of civilization. The United States had a “superior culture” to which the Italians were expected to adapt if they were to succeed.⁴³ This negative perception resulted in various types of social and economic discrimination and ultimately in restrictions on Italian immigration. And it reinforced the limitations created by more restricted economic opportunity.

The differences in the cultures of the two receiving societies also had an impact on immigrant adjustment, but these differences were probably of less importance than some scholars have suggested. In Buenos Aires, the Italians entered a Latin culture whose values, language, and religion were similar to their own. Italians in New York, however, confronted a very different culture; there they had problems with the language, were subjected to religious prejudice, and had difficulty understanding some of the values of the host society. As a result, the Italians in Buenos Aires presumably were able to adjust more easily and their success came more rapidly.⁴⁴ Although the cultural explanation is of some significance, it has its limitations. Most important, the Italians in Buenos Aires were successful in economic life precisely because they rejected some important Argentine values. Economic opportunity and the host society’s perceptions were more significant.

Furthermore, Italians in Buenos Aires inherited a more extensive, wealthier, and more effective organizational network that had been developed by earlier immigrants. When massive migration began in the 1880s, Buenos Aires already had an Italian community of eighty to ninety thousand immigrants. Its established institutions were capable of helping the newcomers adjust to life in the city. In New

⁴² Scobie, *Argentina*, 174–91; Germani, “Mass Immigration and Modernization in Argentina,” 309–11; and Carl Solberg, *Immigration and Nationalism: Argentina and Chile, 1890–1914* (Austin, 1970), 65–116.

⁴³ Pozzetta, “The Italians of New York City, 1890–1914,” 121–60; and John Higham, *Strangers in the Land: Patterns of American Nativism, 1860–1925* (New York, 1975), 68–194.

⁴⁴ Solberg, “Mass Migrations in Argentina,” 146–70; Maxine Seller, *To Seek America: A History of Ethnic Life in the United States* (Englewood Cliffs, N.J., 1977), 105–11; and Tomasi and Engel, *The Italian Experience in the United States*, 77–107.

TABLE 1
Foreign-Born Italians in New York City

<i>Year</i>	<i>City's Italian Population</i>	<i>Italian Percentage of City's Total Population</i>	<i>Annual Growth of City's Italian Population during Preceding Decade</i>	<i>New York City's Italians as a Percentage of All Italians in the U.S.</i>
1860	1,464	0.1		12.5
1870	2,794	0.3	9.1	16.3
1880	12,223	1.0	33.7	27.6
1890	39,951	2.6	23.3	21.9
1900	145,433	4.2	26.3	30.0
1910	340,765	7.1	13.4	25.4
1914	370,000 (estimate)			
1920	390,832	7.0		24.3
1930	440,200	6.4		24.6

NOTE: In 1898, New York City—which until that time had comprised only Manhattan and part of the Bronx—incorporated its neighboring counties to assume its current boundaries: Manhattan, Brooklyn (King's County), Queens (Queens County), Richmond (Staten Island), and the Bronx. Thus, the pre- and post-1900 figures refer to different geographical boundaries of the city.

SOURCES: U.S. Censuses, 1860 to 1930.

York, when massive migration began in the 1890s, the Italian community of forty thousand immigrants did not have as developed or effective an organizational infrastructure and was therefore less able to help subsequent immigrants find their way in their new environment.⁴⁵ Thus, the differences in the economies, the host societies' perceptions, cultures, and immigrant institutions worked together to make adjustment in Buenos Aires relatively easier and more rapid.

In addition to the skills and attitudes the Italian immigrants brought with them and the characteristics of the receiving environments, the subsequent development of the two Italian communities influenced adjustment. Continuing immigration in the 1890s and early 1900s modified existing conditions within the communities, and the new conditions themselves became important in the relative speed and degree of immigrant adjustment. Among the most important of these new conditions were the pace of immigration, the concentration and numerical strength of the two groups, and the nature of the emerging Italian elites.

The pace of migration to the two cities varied considerably. The flow of individuals to Buenos Aires was spread out more evenly over the four decades following 1870 than it was to New York. In 1870, forty-four thousand Italians lived in Buenos Aires, and less than three thousand lived in New York. The Buenos Aires community grew gradually over the next forty-four years; from 1887 to 1914 the annual increase averaged less than 4.3 percent. The New York community, which started with a much smaller base, grew more rapidly throughout the period. Most significantly, 77 percent of all Italian immigrants who went to the United States between 1860 and 1914 arrived during the fifteen years prior to World War I compared to 50 percent of those who went to Argentina (see Table 3). The larger initial size of the Italian population and the greater effectiveness of immigrant organizations combined with the more even pace of subsequent migration enabled the existing community in Buenos Aires to absorb the newcomers more easily and to help them adjust more readily. But the massive influx of immigrants in a short period of time overwhelmed the less developed and less united existing community in New York and prohibited it from absorbing and guiding the new arrivals.

The consistent relative concentration and numerical strength of the Italians in Buenos Aires further strengthened their position. In 1910 the one and one-third million Italians in the United States represented 1.5 percent of the total population. In Argentina the slightly more than one million Italians in 1914 represented 12.5 percent of the total population. Italians in both countries were concentrated in a relatively few states or provinces and cities, but this concentration, too, was far greater in Argentina. One-third of all of the Italians in Argentina lived in Buenos Aires, and the population of the city was at least 20 percent Italian-born throughout the period. Although approximately one-quarter of all of the Italians in the United States lived in New York, they never accounted for more than 7.1 percent of the city's population (see Table 1). In addition, there were only two major immigrant

⁴⁵ Baily, "Las Sociedades de ayuda mutua," 485–514; and Pozzetta, "The Italians of New York City, 1890–1914," 231–66.

<i>Year</i>	<i>City's Italian Population</i>	<i>Italian Percentage of City's Total Population</i>	<i>Annual Growth of City's Italian Population during Preceding Period</i>	<i>Buenos Aires's Italians as a Percentage of All Italians in Argentina</i>
1856	11,000	12.0		
1869	44,000	23.6	23.0	
1887	138,000	31.8	12.0	
1895	182,000	27.4	3.9	36.9
1904	228,000	24.0	2.8	
1909	277,000	22.5	4.3	
1914	312,000	19.8	2.5	33.6
1936	299,000	12.4		

NOTE: The number of Italians in Buenos Aires has been rounded off to the nearest thousand.
 SOURCES: Buenos Aires and Argentine Censuses, 1856 to 1936.

TABLE 3
Italian Immigration to Argentina and the United States

<i>Period</i>	<i>Argentina</i>	<i>United States</i>
1861–70	113,554	11,728
1871–80	152,061	55,759
1881–90	493,885	307,309
1891–1900	425,693	651,899
1901–10	796,190	2,104,209
1911–20	347,388	1,165,246
TOTAL IMMIGRANTS, 1860–1914	2,270,525	4,083,000
TOTAL IMMIGRANTS, 1900–1914	1,137,475	3,156,000
IMMIGRANTS 1900–14 AS A PERCENTAGE OF IMMIGRANTS 1860–1914	50	77

SOURCES: República Argentina, Dirección de Inmigración, *Resumen estadística del movimiento migratorio en la República Argentina, 1857–1924* (Buenos Aires, 1925); and U. S. Bureau of the Census, *Historical Statistics of the United States, Colonial Times to 1972* (Washington, 1975).

groups in Argentina compared to at least a half-dozen in the United States: Italians and Spaniards alone made up nearly 80 percent of the total number of immigrants in Buenos Aires before World War I, whereas Russians, Germans, Irish, Austrians, and Italians made up a similar percentage of the total immigrant population of New York in 1900 and 1910.⁴⁶ The sheer numerical strength of the Italians in Buenos Aires made them far more difficult to exclude or play off against other nationalities, and their position thus guaranteed them more influence in the local society.

The nature of the emerging Italian elites also influenced adjustment patterns. Only two functioning social classes developed within the Italian community in New York: the *prominenti*, the tiny group of successful businessmen and professionals who remained in the community, and the mass of blue-collar workers. The differences in the interests of these two groups were too great for the *prominenti* to provide effective leadership for the mass of laborers. There was no strong middle class to provide pressure for reform or to link these two groups. The occupational structure in Buenos Aires, however, provided the basis for the growth of a multiclass society within the Italian community. The middle groups—the skilled and white-collar workers—were more influential in the organizations of the

⁴⁶ Buenos Aires, *Censo general . . . , 1904*, 28; and U.S. Bureau of the Census, *Thirteenth Census of the United States . . . , 1910*, 827.

community and, as part of a broader elite, were able to articulate and serve the interests of all Italians.⁴⁷

The differences between the two elites can be seen in a number of areas. The mutualist movement and the leading Italian newspapers in Buenos Aires, unlike those in New York, consistently defended the interests of the blue-collar workers as well as those of the rest of the community. But the differences extended to the degree of cooperation among Italians who originated from different regions. In Buenos Aires both northern and southern Italians joined the same organizations and worked together in ways that they did not in New York. The greater numerical equality of the two groups in Buenos Aires obviously facilitated this cooperation, as did the relative lack of prejudice against southerners in the host society. In New York, the small group of northern Italians sought to escape the negative U.S. stereotype by separating themselves from the southern Italians. But the commitment of the early leaders of major mutual aid societies to a united Italy and the continual reinforcement of this commitment by the leaders of the community were of major importance in the greater ability of Italians from all areas to work together in Buenos Aires. What regional hostility there was never prevented Italians of all origins from working together in the same organizations and from uniting in a community that sought to benefit the interests of all.⁴⁸

In New York the Italians developed two social strata in what was a multiclass society; in Buenos Aires they developed a multilevel social structure in what had been essentially a two-class society. This major difference, along with some of the social, economic, and historical differences, resulted in leadership in Buenos Aires that was more representative, more unified, and better able to develop an effective immigrant institutional structure than that in New York. The interaction among the various social sectors of the two communities was distinct and forms an important part of the explanation of the differing experience.

THE MORE RAPID AND MORE SUCCESSFUL ADJUSTMENT of the Italians in Buenos Aires was dependent upon the skills and attitudes the immigrants brought with them, the characteristics of the receiving societies, and the changing nature of the immigrant communities. All of these variables were interrelated. Although the characteristics of the immigrants and the receiving societies were of primary importance at the time of the immigrants' arrival, these two sets of original variables produced a new set of variables that, once in existence, took on a life of its own. The interrelationship of these three categories of variables is fundamental to an understanding of the process of Italian adjustment.

The labor market in Buenos Aires attracted a relatively more skilled and permanent group of Italians who were perceived by the Argentine elite in more positive terms and who encountered a more developed and effective immigrant

⁴⁷ Scobie, *Buenos Aires: Plaza to Suburb*, 208–10; Baily, "Las Sociedades de ayuda mutua," 485–514; and Pozzetta, "The Italians of New York City, 1890–1914," 231–44. Also see note 14, page 286, above.

⁴⁸ Pozzetta, "The Italians of New York City, 1890–1914," 127–28; and Baily, "Las Sociedades de ayuda mutua," 485–514, and "Italians and Organized Labor" (1967), 55–66.

institutional structure. Thus they had an initial advantage over the Italians in New York as they sought to find jobs and housing, to improve their living conditions, and to organize to protect their collective interests. But once in Buenos Aires the Italians capitalized on their initial advantages to strengthen further the position of the Italian community relative to that in New York. Not only did the Italians in Buenos Aires move into the economy in positions with higher status, but they continued to find better jobs and housing and to develop community organizations. Their efforts were facilitated by the gradual pace of immigration and the concentration and relative numerical strength of the group as a whole. At the same time, the Italian multiclass social structure in Buenos Aires provided the basis for a more representative and effective group of leaders.

I have posited a model of the process of adjustment of Italian immigrants to the Americas based on the important cases of Buenos Aires and New York City. These two cases may well represent the extremes in terms of the speed and success of the process. What we need to do now is to use the model to study other cities and other immigrant groups. Much has, of course, been written on immigrants in cities throughout the world. Unfortunately, few of these studies are comparable. It is my strong conviction that historians of immigration who wish to develop their field successfully must frame their research in such a way as to make the results more readily useful to others. My hope is that the model of the adjustment process developed in this essay will enhance this effort. As the model is systematically applied to other cities and other immigrant groups, we will obtain a sufficient number of comparable cases to be able to understand with greater certainty the fundamental nature of the process itself.

AHR Forum
The Integration of Italian Immigrants
into the United States and Argentina:
A Comparative Analysis

HERBERT S. KLEIN

ARGENTINA AND THE UNITED STATES were the two nations that absorbed the largest number of Italian immigrants during the period of Europe's greatest intercontinental migration. Both began to attract Italians on a large scale from the decade of the 1880s, and both continued to accept them in large numbers until the outbreak of World War I and again in the early 1920s. Although the timing, intensity, and peak of Italian immigration differed somewhat in the two nations, their migration flows occurred within largely similar bounds. Yet, despite this common historical evolution, Italian integration and mobility within the two societies differed markedly during the first generation and continues to differ substantially to the present day. The dual aim of this very preliminary survey is to determine the nature of this differing experience of mobility and to suggest factors that explain its evolution. This will involve a study of the regional origins of the Italian immigrants as well as their comparative American integration in terms of occupational distribution, social mobility, and relative wealth.

The sharp differences in the Italian immigrant experience within Argentina and the United States were fully perceived by both the immigrants themselves and virtually all contemporary observers. Most contemporary analysts stressed the relative economic conditions in the two host countries as the main explanatory factor. But in more recent years several alternative causal models have been proposed.¹ A cultural model emphasizing the attitudes and background of the arriving migrants has again become popular among historians.² Among econo-

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¹ For a good survey of this literature, see J. D. Gould, "European Inter-Continental Emigration, 1815–1914: Patterns and Causes," *Journal of European Economic History*, 8 (1979): 593–639.

² In the United States, the cultural emphasis can be found in such recent studies as Stephan Thernstrom's *The Other Bostonians: Poverty and Progress in an American Metropolis, 1880–1970* (Cambridge, Mass., 1973), and Thomas Kessner's *The Golden Door: Italian and Jewish Immigrant Mobility in New York City, 1880–1915* (New York, 1977). For the older economic model, see, for example, Robert F. Foerster, *The Italian Emigration of Our Times* (Cambridge, Mass., 1919); and Humberto Nelli, *The Italians in Chicago, 1880–1930* (New York, 1970).

mists, debate about bias in labor markets has led to a series of alternative hypotheses to explain immigrant integration. U.S. economic historians have debated the existence of prejudice against immigrants in determining wage differentials and have even suggested regional wage and labor-market variations as important explanatory variables.³ Finally, recent work on international migrations has led to the elaboration of a dual labor-market model. This postulates a basic demand function in advanced industrial societies for unskilled labor entering into high-risk and low-status occupations and industries, a market only supplied by nonnative migrants. The emphasis in the dual labor-market theory on immigrant strategies in relation to status and savings in the mother and host countries is especially relevant to the Italian experience.⁴

Testing both the older and more recent models found in the literature on migration and integration requires surveying statistical materials to determine the regional differences within Italy, the differences in the migration streams to Argentina and the United States as seen by American immigration officials, and the differences in the rates of occupational and social mobility in the host countries.

ALTHOUGH ARGENTINA AND THE UNITED STATES were the two primary countries of Italian immigration in the Americas between 1880 and 1914, the flows of Italian migrants to their shores differed over time. It was Brazil and Argentina that initially were the primary targets of Italian migrants, with the United States running a poor third. In the 1870s, and through most of the 1880s, Argentina was the primary zone of reception. By the late 1880s, with the abolition of slavery and the massive shift to subsidized Italian workers in the expanding coffee fields of São Paulo, Brazil temporarily emerged as the primary immigration zone despite the steady increase in Italian migration to both the United States and Argentina. But the hardships imposed on the Italian coffee workers led the Italian government to oppose Brazilian subsidization, and by the early 1900s Italian migration to Brazil was halved, while it continued to increase to the other two states.⁵ By 1900 the

³ The debate began with Robert Higgs's "Race, Skill, and Savings: American Immigrants in 1909," *Journal of Economic History*, 31 (1971): 420–28. Higgs stressed that, once differences in skills, literacy, and length of residence are controlled for, there was no discrimination in wage payments to immigrants. For work that supports Higgs's position, see Peter J. Hill, "Relative Skill and Income Levels of Native and Foreign-Born Workers in the United States," *Explorations in Economic History*, 12 (1975): 47–60; Peter R. Shergold, "Relative Skill and Income Levels of Native and Foreign-Born Workers: A Re-Examination," *ibid.*, 13 (1976): 451–61; Martha Norby Fraundorf, "Relative Earnings of Native and Foreign-Born Women," *ibid.*, 15 (1978): 211–20; and Michael B. Tannen, "Women's Earnings, Skill, and Nativity in the Progressive Era," *ibid.*, 19 (1982): 128–55. Only Paul L. McGouldrick and Michael B. Tannen have disagreed, arguing that a 10 percent wage differential between southern and eastern Europeans and the older immigrants and native-born can still be seen even after controlling for skill, literacy, and length of residence; McGouldrick and Tannen, "Did American Manufacturers Discriminate against Immigrants before 1914?" *Journal of Economic History*, 37 (1977): 723–46. For a discussion of regional differences in labor markets within the United States, see Gordon W. Kirk and Carolyn T. Kirk, "The Immigrant, Economic Opportunity, and Type of Settlement in Nineteenth-Century America," *ibid.*, 38 (1978): 226–34.

⁴ See Michael J. Piore, *Birds of Passage: Migrant Labor and Industrial Society* (Cambridge, Mass., 1979).

⁵ For a good survey of Italian immigration to Brazil, see Angelo Trento, "Misericordia e speranza: L'Emigrazione italiana in Brasile, 1887–1902," in José Luiz del Rio, ed., *Lavoratori in Brasile* (Milan, 1981). Also useful are Franco Cenni, *Italiani no Brasil* (São Paulo, 1975); and Antonio Franceschini, *L'Emigrazione nell'America del Sud* (Rome, 1908), chaps. 4–5. On the history of the early experimentation with immigrant labor and the transition

TABLE 1
Estimate of Annual Italian Migration to the Americas, 1876–1914

<i>Period</i>	<i>Argentina</i>	<i>Brazil</i>	<i>United States</i> ¹	<i>Other</i>	<i>Total</i>
1876–80	8,871	3,722	2,675	11,067	26,335
1881–85	26,532	8,371	14,952	8,527	58,382
1886–90	51,769	34,739	34,094	9,818	130,420
1891–95	31,117	65,981	41,319	8,374	146,791
1896–1900	42,247	50,064	61,546	7,152	161,009
1901–05	55,702	40,021	199,670	11,702	307,095
1906–10	91,217	20,652	266,220	13,826	391,915
1911–14	62,799	25,954	250,745	22,296	361,794

SOURCE: Istituto Centrale di Statistica, *Bolettino mensile de Statistica* (Gennaio, 1975), Anno 5, n. 1, Appendix 2: "Espatriati e Rimpatriati, anni 1876–1973," pp. 254–55. These statistics represent the latest Italian effort to measure migration and appear to be a judicious blend of the older DGS and CGE (Statistical Bureau and Emigration Commissariat) series. For a useful critique of these earlier series, see Marcello Carmagnani and Giovanna Mantelli, "Fonti quantitative italiane relative all'emigrazione italiana verso l'America Latina (1902–1914)," *Annali della Fondazione Luigi Einaudi*, 9 (1975).

United States had finally emerged as the major recipient of Italians arriving in America. Once this domination was achieved, it was never challenged. As of the outbreak of World War I, the United States had absorbed close to 70 percent of all immigrants going to these three primary American host countries. (See Table 1.)

Not only was the timing of the flows different among the three receiving countries, but the regional origins of the immigrants were also varied, which reflected changing regional interest in the overseas migration process, as southerners, especially, became more committed to overseas migration in the last decade of the nineteenth century and the first two decades of the twentieth century.⁶ But equally there were within the regions of Italy clearly marked preferences for specific American countries, which also influenced the flow of migrants.

Even in the earliest stages of the massive migration period, southern Italians showed special interest in the United States and less interest in Argentina; equally, while northern and central Italian migrants went to the United States, they showed a more general interest in Argentina as their destination (see Table 2). As the

from slave to free workers, see Emilia Viotti da Costa, *Da senzala à colônia* (São Paulo, 1966); and Warren Dean, *Rio Claro: A Brazilian Plantation System, 1820–1920* (Stanford, 1976). The best study to date on the economic life of the immigrants is Thomas Holloway, *Immigrants on the Land: Coffee and Society in São Paulo, 1886–1934* (Chapel Hill, 1980). Finally, for an interesting comparative treatment of the distortions in the immigration flow caused by Brazilian subsidization, see Chiara Vangelista, "Immigrazione, struttura produttiva, e mercato del lavoro in Argentina e in Brasile (1876–1914)," *Annali della Fondazione Luigi Einaudi*, 9 (1975): 197–216; also see her *La Braccia per la fazenda: Immigrati e "caipiras" nella formazione del mercato del lavoro paulista (1850–1930)* (Milan, 1982).

⁶ Pre-World War I regional divisions are used throughout: northern Italy comprises Piemonte, Liguria, Lombardia, and Veneto; central Italy comprises Emilia, Toscana, Marche, Umbria, and Lazio; and southern Italy comprises the remaining peninsular divisions and the islands.

TABLE 2
Regional Origins of Italian Immigrants, 1876–1930

<i>Period</i>	<i>Percentage from Northern Italy</i>	<i>Percentage from Central Italy</i>	<i>Percentage from Southern Italy</i>
<i>Argentina</i>			
1876–78	66	6	28
1884–86	66	9	25
1894–96	44	16	39
1904–06	32	20	48
1907–09	31	14	55
1910–14	31	14	54
TOTAL, 1876–1930	41	12	47
TOTAL IMMIGRANTS, 1876–1930	988,235	281,577	1,116,369
<i>United States</i>			
1876–78	41	11	47
1884–86	12	3	85
1894–96	7	5	88
1904–06	9	12	79
1907–09	9	14	78
1910–14	11	14	75
TOTAL, 1876–1930	11	9	80
TOTAL IMMIGRANTS, 1876–1930	564,345	460,227	4,034,204

NOTE: The regional divisions used here are those in effect before 1919—that is, northern Italy includes Piemonte, Liguria, Lombardia, and Veneto; central Italy includes Emilia, Toscana, Marche, Umbria, and Lazio; and southern Italy includes the remaining peninsular divisions plus the islands.

SOURCES: Commisariato Generale dell'Emigrazione, *L'Emigrazione italiana dal 1910 al 1923* (Rome, 1926), 832–49; and Associazione per lo sviluppo dell'industria nel mezzogiorno [hereafter, SVIMEZ], *Statistiche sul mezzogiorno d'Italia, 1861–1953* (Rome, 1954), 118.

volume of southern migration continued, however, it appears that southerners were discriminating more in the areas of destination, and thus the percentage going to South America increased considerably, while the ratio going to the United States declined. This was occurring as overall southern Italian migration was increasing steadily (see Table 3). Thus, prior to World War I, sharp regional preferences were beginning to break down as all regions sent migrants to all American states. But this

TABLE 3
Regional Origins of the Transatlantic Italian Immigrants, 1876-1914

Period	Percentage from Northern Italy	Percentage from Central Italy	Percentage from Southern Italy	Total Number of Transatlantic Migrants	Transatlantic Migrants as a Percentage of All Migrants
1876-79	61	6	33	99,722	24
1880-84	45	7	48	254,750	35
1885-89	39	11	50	514,949	54
1890-94	32	11	57	566,260	51
1895-99	29	11	60	787,521	54
1900-04	13	12	74	1,265,632	53
1905-09	15	13	71	2,012,774	60
1910-14	17	14	69	1,861,644	57
Total, 1876-1914	26	10	64	7,363,252	55

NOTE: The regional divisions used here are those that obtained prior to 1919.

SOURCE: Commissariato Generale dell'Emigrazione, *L'Emigrazione italiana dal 1910 al 1923*, *passim*.

process never reached total equality, and the United States undoubtedly received more southern Italians than did Argentina overall, although both countries had immigration flows from all three Italian regions.⁷

With southern Italians accounting for 80 percent of the total immigration of Italians to the United States in the 1876–1930 period, and only 47 percent to Argentina, it is worth exploring the regional variations between the two major Italian regions of the *nord* (central and northern districts) and the *mezzogiorno* (southern departments) to determine the nature of the differences, if any, that existed between the migrants from these two basic regions within Italy. What becomes apparent from this comparison is that Italy itself was made up of so many complex, even intraregional, economic and social structures that the statistics reveal relatively little variation across regions despite the fact that the *mezzogiorno* was clearly less developed than the *nord*.

Thus a comparative analysis of the major demographic variables reveals only modest variation between the *nord* and *mezzogiorno*. In terms of age structure and civil status, there is virtually no difference between the two regions. Age at marriage, birth rates, and even size of families were all quite similar across Italy. It is only in such categories as still births, infant mortality, crude death rates, and average heights that we can begin to see a distinction between the two zones. Evidently, the *mezzogiorno* was not as healthy a region as the *nord*, not in terms of climate but rather because of differences in wealth. This is obvious from the 1928 income differentials. It is also apparent from the wide differences in educational opportunities in the two regions, which found expression in the extreme difference in literacy rates. (See Table 4.)

But, despite the wealth-poverty distinction, one is still surprised by the relatively modest variation in most of the demographic and even the economic statistics. Especially surprising is the commonality of the divisions by major occupational categories of the economically active populations. Thus, while there was obviously an important *nord-mezzogiorno* distinction, it is not as sharp as the one found today in most Third World countries, which have an advanced-backward regional split.

Moreover, as many commentators have stressed, there was a tendency for selectivity of immigrant groups in all zones. It was obviously not the poorest and least prepared groups that migrated from either the *nord* or the *mezzogiorno*. Equally, given the very complex land, labor, and social arrangements within each region, it is not surprising that peasants or urban dwellers—north, center, or south—came from quite similar backgrounds and experiences. All recent studies stress that the immigrant groups usually came from the better situated and more mobile upper elements of the working classes in all regions, thus tending toward a homogenization of immigrants, despite the regional variations that did exist.⁸

⁷ For useful surveys of the changing directions of Italian migration in this period and its context within a century of intense Italian migration, see Luigi Favero and Graziano Tassello, "Cent'anni di emigrazione italiana (1876–1976)," in Gianfausto Rosoli, ed., *Un Secolo di emigrazione italiana* (Rome, 1978), 9–63; and Ercole Sori, *L'Emigrazione italiana dall'unità alla seconda guerra mondiale* (Bologna, 1979), chap. 2.

⁸ Sori, *L'Emigrazione italiana*, 295–96; and J. S. McDonald, "Some Socio-Economic Differentials in Rural Italy, 1902–1913," *Economic Development and Cultural Change*, 7 (1958).

TABLE 4
Comparative Characteristics of the Italian Population

	<i>Nord</i>	<i>Mezzogiorno</i>
AGE STRUCTURE (1901)		
0–14 years	34 percent	35 percent
15–39 years	37 percent	36 percent
40–59 years	19 percent	20 percent
60+ years	10 percent	10 percent
SEX RATIO (1901)		
Male to Female	100.5	96.8
VITAL STATISTICS (1910–12)		
Crude Birth Rate	31.4	34.0
Age-Specific Birth Rate per 1000 women, 14–45	269	271
Still Births per 1000 live births	39.8	43.6
Infant Mortality (deaths under 1 year per 1000 live births)	137.4	147.3
Crude Death Rate	18.5	21.9
Natural Growth Rate	12.9	12.2
CIVIL STATUS (1901)		
Single	33 percent	30 percent
Married	54 percent	57 percent
Widowed	13 percent	12 percent
FAMILY STRUCTURE (1913–14)		
Average Age at Marriage (Males)	27.6 years	27.1 years
Average Age at Marriage (Females)	23.8 years	23.3 years
Average Size of Family (1911)	4.8 persons	4.1 persons
HEIGHT OF MALE CONSCRIPTS		
	165.03 cm.	161.83 cm.
TOTAL POPULATION		
1881	17.3 million	11.2 million
1901	19.7 million	12.7 million
1911	21.4 million	13.3 million
ESTIMATED NET MIGRATION LOSS		
1881–1901	1,250 thousand	930 thousand
1901–11	787 thousand	859 thousand
DISTRIBUTION OF MALE WORKFORCE, 1901		
Agriculture	57 percent	62 percent
Industry	25 percent	21 percent
Transportation	4 percent	4 percent
Commerce	6 percent	5 percent
Other	8 percent	8 percent
INDEX OF AVERAGE PER CAPITA INCOME, 1928 (Italy = 100)		
	117.2	69.5
ILLITERACY RATE, 1901		
Persons above 5 years of age	33.9 percent	68.1 percent

SOURCES: SVIMEZ, *Statistiche sul mezzogiorno, passim*; and Istituto Centrale di Statistica, *Sviluppo della popolazione italiana dal 1861 al 1961* (Rome, 1965), 56–63, 301.

TABLE 5
Argentine and U.S. Immigrant Occupations (as a Percentage), 1876–1910

Occupation	Argentina (1876–1909)		United States (1899–1910)	
	Italians	All Immigrants	Italians	All Immigrants
AGRICULTURAL WORKERS AND FARMERS: <i>agricultores</i>	68	56	33	25
UNSKILLED DAY LABORERS: <i>jornaleros</i>	13	18	43	36
SKILLED AND SEMI- SKILLED WORKERS: <i>artesanos y artistas</i>	10	12	16	22
MERCHANTS: <i>comerciantes</i>	2	5	1	2
OTHER PROFESSIONS: <i>varios profesiones</i>	6	9	6	16
NUMBER OF IMMIGRANTS, OCCUPATION LISTED	1,475,073	2,644,642	1,768,281	7,049,010
NUMBER OF IMMIGRANTS, NO OCCUPATION LISTED	208,888	436,906	516,320	2,506,717

SOURCES: Direccion Nacional de Migraciones, *Memorias Anuales, 1899–1910*; and U.S. Immigration Commission [Dillingham Commission], *Reports*, volume 3: *Statistical Review of Immigration, 1820–1910* (Washington, 1911), 98–178.

The question therefore remains as to what differences actually existed in the stream of migrants arriving in the two American nations. Differences were, in fact, noted by U.S. and Argentine immigration officials. Thus the moderate variations between migrants from the *nord* and those from the *mezzogiorno* seen in the Italian statistics are far more exaggerated in the American data. Argentina, for example, appears to have received far more agricultural workers and far fewer day laborers than did the United States. While farmers and farm laborers made up just a third of the Italians arriving in the United States, they made up over two-thirds of the Italians arriving in Argentina. Equally, while day laborers were the dominant occupational group of immigrants arriving in the United States, they made up only 13 percent of the arrivals in Argentina. (See Table 5.) This difference in the labeling of unskilled workers probably reflects the relative timing of the two migration streams. Up until the 1890s there was an overwhelming predominance of agricultural workers listed among the unskilled emigrating Italians, whereas after 1900 this imbalance tended to disappear as both groups of occupations were more nearly equal in representation among the male migrants leaving Italy. (See Table 6.)

But the entire distinction between nonfarm unskilled laborers and farm workers may have been rather artificial to begin with. Even as late as 1911, 60 percent of the male labor force in Italy was still engaged in agriculture. As the leading contemporary expert on Italian immigration noted at the time, the majority of unskilled workers, no matter what their designation, in fact came from the rural population

and should equally be counted as agricultural laborers.⁹ Thus, it can be assumed that most of the arriving *giornaliere* (*jornaleros*, or “day laborers”) were of agricultural origin and could have been employed in farm work. It is justifiable, then, to place these two groups together when assessing skilled and unskilled occupations of arriving immigrants in the two American nations. When this is done, the combined totals of farm and day laborers are quite similar in the two host countries: 81 percent for Argentina and 76 percent for the United States. What makes for the major differences between Italians going to Argentina or the United States, then, is not so much the relative importance of unskilled, skilled, and professional workers, which did not differ significantly, but between the number of unskilled workers listed as doing agricultural or day labor.

What this distinction signifies is, however, difficult to assess fully. As Roberto Cortes Conde has recently pointed out for Argentina, the mobility of workers

TABLE 6
Occupations of Italian Adult Male Migrants (as a Percentage), 1876–1910

Period	Agricultural Laborers and Farmers	Unskilled Day Laborers	Skilled Construction Workers	Industrial Workers and Artisans	Liberal Professions	Other and Unknown
1876–78	42	21	16	12	0.7	8
1884–86	50	23	12	8	0.6	6
1894–96	45	26	17	6	0.6	5
1904–06	36	32	13	12	0.3	7
1907–10	35	33	13	11	0.4	7

NOTE: “Adult” is here defined as fifteen or more years old.

SOURCE: Francesco Coletti, *Dell'emigrazione italiana* (Milan, 1912), 55.

between the categories of urban day laborers and construction workers on the one hand and agricultural laborers on the other was quite high at the end of the nineteenth century. There seemed little difficulty in Argentina, at least, of *jornaleros* passing over temporarily into *peones* in the rural sector during harvest periods, particularly when there was unemployment in the construction and urban sectors.¹⁰ In the United States this possibility does not appear to have occurred. In Argentina the high cost of attracting native Argentine workers out of subsistence and into commercial agriculture contrasts with the situation in the United States, in which Americans were fully integrated into commercial farming and expanding demand was met with a native-born labor force. Thus the undifferentiated nature of these two occupational categories, implicit in the Italian and Argentine experience, could

⁹ In 1912 Francesco Coletti declared that he and all other analysts at that time found that “laborers, day laborers, and the like come in large part from the rural classes and for that reason should be added to the category of agricultural laborers in order to account fully for the rural contingent in the emigrant stream”; Coletti, *Dell'emigrazione italiana* (Milan, 1912), 56.

¹⁰ Cortes Conde, *El Progreso argentino, 1880–1914* (Buenos Aires, 1979), 197–204.

not be tested in the United States because of the competition of the native-born in the agricultural market.¹¹

Whatever the real differences may have been between these occupational definitions, it is worth stressing that those going to Argentina reflected quite closely all foreign immigration going to that country, just as the occupational breakdown of those going to the United States reflected all foreign immigrations in that period (see Table 5). In both cases, Italians were either more heavily agricultural workers or day laborers than the total migration group. But in both cases, for Italians and total immigration, there appears a much greater stress in one country on agriculturalists and in another on unskilled day laborers.

Migrants bound for Argentina were apparently more literate than those headed to the United States (although the figures are not quite comparable); but the majority of immigrants in both streams was literate, just as the majority of the Italian population, according to the census of 1901, was literate. Immigrants to both American republics, however, apparently were somewhat more literate as a group than the entire Italian population (see Tables 4, 7, and 8). This finding, as well as the higher percentage of skilled workers noted even for the U.S.-bound immigrants, seems to support the assertion of recent studies that it was the more skilled and educated of the laboring classes in both societies that migrated.¹²

In terms of demographic characteristics, there were differences between the immigrants to the United States and Argentina. Although immigrant groups were dominated by working-age males, the U.S.-bound Italians were considerably more so. But higher repatriation of Italian males than Italian females led to an approximately equal sex ratio in both resident Italo-American communities by 1914 and 1920 respectively (see Tables 7 and 8).¹³ In both cases, however, this ratio was still considerably higher than that for the respective native-born populations, just as the mean age of the Italian-born immigrants in both countries was on average ten to fifteen years older than that of the American-born populations.¹⁴

¹¹ In 1910 the South and the North Central states together had 85 percent of the farm population (as well as 80 percent of all farm acreage and the total value of all farms) but only 20 percent of the Italian population. Moreover, since three-quarters or more of the Italians lived in urban areas, probably no more than sixty thousand Italian immigrants worked in the agricultural heartland of the United States. In contrast, 69 percent of the native-born population lived in these two regions. U.S. Bureau of the Census, *Thirteenth Census of the United States . . . , 1910*, 1 (Washington, 1913): 800, and *Historical Statistics of the United States, Colonial Times to 1970*, 1 (2d edn., Washington, 1976): 90–92, 458.

¹² For the Italian background experiences of selected U.S.-bound immigrants, see John W. Briggs, *An Italian Passage: Immigrants to Three American Cities, 1890–1930* (New Haven, 1978), chap. 1; and Virginia Yans-McLaughlin, *Family and Community: Italian Immigrants in Buffalo, 1880–1930* (New Haven, 1978), chap. 1.

¹³ The only detailed study of the age, sex, and civil status of repatriated immigrants is the model demographic analysis by Massimo Livi Bacci for Italians returning from the United States. He concluded that in the period 1909–28 a much higher percentage of the repatriates was male than was the original U.S.-bound group of immigrants: the sex ratio of the repatriates was 734 males per 100 females, and that for the immigrants was 234 per 100 females. The repatriates were more likely than the immigrants to be single young adults, and they were far more likely to have come from the most recently arrived group of immigrants. Whereas 90 percent of the repatriated Italians had resided in the United States fewer than ten years, only 43 percent of the foreign-born Italians in 1930 had resided in the United States fewer than twenty years. See Livi Bacci, *L'immigrazione e l'assimilazione degli italiani negli Stati Uniti secondo le statistiche demografiche americane* (Milan, 1961), chap. 4.

¹⁴ República Argentina, *Tercer censo nacional de . . . 1914*, 10 vols. (Buenos Aires, 1916), 1: 143. In the United States in 1910, the mean age differential was approximately the same at thirteen years; U.S. Bureau of the Census, *Historical Statistics of the United States*, 19.

TABLE 7
 Characteristics of Italian Immigrants and First-Generation Italo-Americans
 in Comparison to All Immigrants, 1899–1910

	<i>Northern Italians</i>	<i>Southern Italians</i>	<i>All Italians</i>	<i>All Immigrants</i>
OCCUPATIONAL STRUCTURE				
Professionals	1 percent	<1 percent	<1 percent	1 percent
Skilled Workers	20 percent	15 percent	16 percent	20 percent
Farm Laborers	19 percent	34 percent	32 percent	23 percent
Day Laborers	48 percent	8 percent	9 percent	19 percent
Other	12 percent	8 percent	9 percent	19 percent
SEX RATIO (Male to Female)				
Immigrants, 1899–1910	361.2	367.5	366.5	227.8
Immigrants, 1893–1914			325.0	
Resident Foreign-Born, 1910			190.6	131.1
AGE STRUCTURE				
Immigrants, 1899–1913				
Under 14 years	9 percent	12 percent	11 percent	12 percent
15–44 years	87 percent	82 percent	83 percent	82 percent
45+ years	4 percent	6 percent	6 percent	5 percent
Resident Foreign-Born, 1920				
Under 15 years	9 percent	12 percent	11 percent	12 percent
15–44 years			70 percent	56 percent
45+ years			25 percent	40 percent
ILLITERACY RATE				
Persons above 14 years	12 percent	54 percent	47 percent	27 percent
FINANCIAL CONDITION				
Average Savings upon Arrival of Those Declaring Income	\$30.76	\$17.14	\$19.45	\$28.95
Those Arriving with Less than \$30.00, 1899–1903	37 percent	7 percent	n/a	18 percent
Those Arriving with Less than \$50.00, 1904–1914	14 percent	6 percent	n/a	14 percent

SOURCES: U.S. Immigration Commission, *Reports*, 3: 47, 84, 95, 350; Massimo Livi Bacci, *L'Emigrazione e l'assimilazione degli italiani negli Stati Uniti secondo le statistiche demografiche americane* (Milan, 1961), 103; and U.S. Bureau of the Census, *Thirteenth Census of the United States. . . , 1910*, 1 (Washington, 1913): 866, and *Historical Statistics of the United States from Colonial Times to 1972*, 1 (Washington, 1975): 16–17.

TABLE 8
 Characteristics of Italian Immigrants and First-Generation Italo-Argentines
 in Comparison to All Immigrants, 1893–1914

	<i>All Italians</i>	<i>All Foreign-Born Residents</i>
AGE STRUCTURE		
Resident in 1914		
Under 15 years	7 percent	10 percent
15–44 years	61 percent	66 percent
45+ years	32 percent	25 percent
Immigrants, 1893–1909		
Under 13 years	15 percent	15 percent
13+ years	85 percent	85 percent
SEX RATIO (Male to Female)		
Immigrants, 1893–1909	267.8	260.6
Resident Foreign-Born, 1914	171.2	166.7
CIVIL STATUS (Residents, 1914)		
Single	27 percent	36 percent
Married	65 percent	57 percent
Widowed	8 percent	7 percent
ILLITERACY RATE (Residents, 1914)		
Persons 7 years and older	36 percent	32 percent

NOTE: Data are missing for age of immigrants for five years—1901–03 and 1905–06.

SOURCES: República Argentina, *Tercer censo nacional de . . . 1914*, 10 vols. (Buenos Aires, 1916), *passim*; and Dirección General de Inmigración, *Resumen estadístico del movimiento en la República Argentina años, 1857–1924* (Buenos Aires, 1925), 19, and *Memorias Anuales, 1899–1910*.

HOW CAN THE DIFFERENCES between the regional similarities in Italy and the very sharp differences between the two migration streams of Italians arriving in America be explained? It has been argued that the cultural differences between Latin and Catholic Argentina and Protestant and English-speaking United States attracted different kinds of immigrants. Such differences may have influenced some of the elite liberal professionals among the Italian migrants—no more than 1 to 2 percent in either immigrant stream—but cannot explain the self-selection among the mass of immigrants. Clearly, the forms of agricultural production were not dissimilar in the Argentine pampas and the U.S. plains. Wheat production technology was

identical and was easily adopted by the immigrants. In fact, Italian agricultural laborers in South America were required to adapt to coffee plantation agriculture and cattle ranching, both new technologies for them. Within the North American context, they could readily have developed any special skills needed to handle the cereal or other agricultural production techniques then available. Even in industrial pursuits, the world of U.S. industrial activity was not dissimilar to the northern Italian experience of the time, nor did it require a highly trained work force. In short, there was neither a cultural nor a technological impediment to any specific type of Italian immigration, nor was southern or northern Italian work experience very different from what might be required in similar situations in the United States or Argentina.

It would appear, then, that it was primarily differing labor markets within the United States and Argentina that exercised an attraction for distinct types of immigrants and thus distorted the streams of Italians flowing to the New World. Each local economy provided different incentives, and Italians responded to those incentives. As contemporaries were well aware, the relative availability of land in Argentina and its relative scarcity in the United States was one major factor. The second was the relative demand of a beginning industrial plant in Argentina and the concomitant new urban expansion compared to an older and established industrial plant in the United States with its fully elaborated urban complex. Finally, the history of previous immigrations meant that the relative importance of Italian immigrants differed in important respects within the two countries.

In Argentina, the Italians were among the first massive group of immigrants to arrive and could establish immigrant norms of integration. They both dominated the foreign-born group and even formed a substantial minority in the total population (they were 39 percent of the foreign-born residents and 12 percent of the entire population in 1914). In contrast, Italians in the United States were late arrivals and in 1910 ranked fourth among foreign-born, accounting for just under 10 percent of the foreign-born but 1.5 percent of the national population. Although Italians increased in importance to second place among the foreign-born by 1920, the concurrent growth of the native population meant that Italians still accounted for only 1.5 percent of the entire U.S. population. Moreover, of the other major immigrant groups, those accounting for over a million migrants, only the Poles and the Russians were like the Italians in being "new immigrants" of the post-1880 period. The equally important Germans and Irish came well before 1880.¹⁵ Both the Polish and Russian migrants brought the same skills as the Italians, and the first- and second-generation Germans and Irish were already well established before the arrival of the Italians. Indeed, newly arrived Italians could often obtain only those jobs scorned by the native-born and the second-generation children of immigrants. Nor were there any major opportunities in the Eastern half of the United States—by then the oldest industrial sector of the country—for innovative industrial development.¹⁶

¹⁵ *Historical Statistics of the United States*, 105–09.

¹⁶ In 1910 only 11 percent of the Italian immigrants lived in the Middle West industrial states (East Northcentral region), whereas 72 percent were confined to the Northeast. New York State alone had over one-

All this was perfectly evident to both the northern and southern Italians who migrated. Thus, the perceptions they held of the two labor markets determined their decisions to migrate, their willingness to remain in the country, and their ideas about developing a local community. Even patterns of savings and investment were largely determined by their perceptions of the possibilities within the two labor markets.

In the United States, this meant that the immigrants were overwhelmingly single males going into day laborer positions within the urban centers. Italians arrived just as the modernization (lighting, urban rail transport, sanitation, and the like) of the North American cities was occurring. These opportunities, along with more traditional aspects of mining, railroad and highway construction, and housing, all provided excellent labor markets for the Italians. Given the high wages, especially in comparison to those in Italy, the majority of the Italians were able to return to Italy as planned, and thus the development of the local community was considerably slowed. Given the relatively higher wages of manual labor in North America, Italians, unlike native-born Americans, were willing to concentrate on immediate income and ignore both long-term investments in human capital for themselves or their families in America and the low-status assignment of these jobs. This basic decision, even for those who remained, meant that savings, if not repatriated, were placed in factors that were not directly related to long-term occupational mobility—investing more in housing than education, for example. For the first generation, this was a clearly thought-out alternative, as repatriated savings became the basis for occupational mobility at home.

Such a strategy based on short-term expediency was also apparent for many Argentine migrants, as the repatriation figures suggest. Thus, between 1880 and 1920 roughly equal ratios of Italians returned from Argentina as had returned from the United States during this period (or approximately 51 percent for Argentina to approximately 54 percent for the United States).¹⁷ But, given the differing market opportunities in land, commerce, and industry, the possibilities for those who remained in America were greater in Argentina than they were in the United States. Thus investments in land, stores, factories, and children's education were seen to have a high payoff in the long run and to be worth sacrificing immediate consumption or security to obtain. Given the more closed nature of the U.S. market, such sacrifices were not as readily justified. Thus, just as

third of the Italian-born immigrants. U.S. Bureau of the Census, *Thirteenth Census of the United States, . . . 1910*, 800, 804. In Argentina, however, all foreign-born residents, including the Italian immigrants, were concentrated in the litoral region, which was the center of both commercial agriculture and all industrial activity. Cortes Conde, *El Progreso argentino, 1880–1914*, 70.

¹⁷ I calculated the rate of return from the United States using Livi Bacci's differing decennial estimates for the period 1880–1920 to obtain an overall ratio; *L'Immigrazione e l'assimilazione*, 35. I calculated the overall estimate of return from Argentina using the data in Dirección General de Inmigración, *Resumen estadístico del movimiento migratorio en la República Argentina, años 1857–1924* (Buenos Aires, 1925), 8. Recent Italian government statistics, which began to be compiled only in 1905, show even higher trends of repatriation for the period 1905–1920 than the estimates of Livi Bacci for the United States, but these statistics correspond more closely with the official Argentine figures. See Instituto Centrale di Statistica, *Bollettino mensile di Statistica* (Gennaio, 1975), Anno 5, Appendix 2: "Espatriati e Rimpatriati, anno 1876–1973," 255, 263. Because of his detailed calculations for birth and death rates and his reliance on censused resident populations, I prefer Livi Bacci's figures. On the difficulties involved in estimating these rates, see J. D. Gould, "European Inter-

the differences in the labor markets help explain the differences in the characteristics of the arriving migrants, these same variables are fundamental to explaining the savings and investment strategies of those who did not repatriate themselves and their earnings. This in turn goes a long way toward explaining the relative rates of mobility and integration within the two societies.¹⁸

To assess more fully these relative rates, it would be ideal if we could compare the differences in the occupational structure, in land ownership, and in ownership of the means of production. Unfortunately, such a comparable set of figures does not yet exist for either society. Moreover, in each nation different sets of materials exist, which are not quite comparable. In Argentina, for example, there are excellent ownership data by the first part of the twentieth century, but comparatively poor occupational data. In the case of the United States, there exist excellent occupational and income data, but ownership data are poor. Equally, although much of the Argentine data is national in scope, most of the U.S. data is found only in sample surveys of selected regions. But it will be argued, as this analysis proceeds, that enough material exists to make a rough approximation of mobility in the two societies.

THE MAIN ATTRACTION FOR MANY ITALIAN IMMIGRANTS going to Argentina was the relative availability of land and the possibility of entering the role of independent farmer. Although traditionally it was assumed that a few Argentines monopolized the land market, it has recently been shown that an open land market existed in Argentina from at least 1880 onward. At the same time, the stock of cultivated land after 1880 was increasing by an extraordinary 10 percent per year.¹⁹ Although the relative returns between renting and owning may have favored the former activity to the discouragement of property ownership, there is little question that Italians became farm owners in unusually large numbers for such a recently arrived immigrant group.

Native Argentines naturally continued to monopolize the land market, but, as the data of the 1914 census indicate, a remarkably large proportion of Italians

Continental Emigration—The Road Home: Return Migration from the U.S.A.," *Journal of European Economic History*, 9 (1980): 79–87. Brazilian-bound Italian immigrants also repatriated at comparable rates during the period 1901–20; see G. Mortara, "A imigração italiano no Brasil e algumas características do grupo italiano de São Paulo," *Revista Brasileira de Estadística*, 11 (1950): 325.

¹⁸ Even in political terms, the Italian experience differed strongly in the two societies, as studies of radical politics and labor organization show; see Samuel L. Baily, "The Italians and Organized Labor in the United States and Argentina, 1880–1910," *International Migration Review*, 1 (1967): 55–66. It is worth pointing out, however, that for the Italians who remained in America, their social integration with the native population proceeded in roughly comparable rates for both Argentina and the United States. Although historians have debated the relative cultural homogeneity or acculturation of the Italian immigrant communities, demographic studies for both Argentina and the United States suggest that Italians moved very rapidly toward native national norms in rates of fertility and nuptiality and also that they intermarried with the native-born populations at ever-increasing rates. See Livi Bacci, *L'immigrazione e l'assimilazione*, chap. 6; and Samuel L. Baily, "Marriage Patterns and Immigrant Assimilation in Buenos Aires, 1882–1923," *Hispanic American Historical Review*, 60 (1980): 32–48.

¹⁹ Cortes Conde, *El Progreso argentino*, 239. In contrast, U.S. farm acreage grew by only 4.6 percent in the entire decade 1901–10; U.S. Bureau of the Census, *Historical Statistics of the United States*, 457.

succeeded in owning land.²⁰ Italians were consistently the second most important group of landowners in cereal and cattle production and were especially successful in grain farming. In fact, Italian-born immigrants within Argentina did extremely well in landownership in general throughout the republic. As of the 1914 census, 25 percent of the Italians twenty or more years of age owned some type of property, a figure close to the national norm and only slightly below that for native-born Argentines. Italians also did far better than the Spaniards, who were almost as numerous and were the second most important immigrant group in the nation. (See Table 9.)

This high general rate of property ownership among Italians is partially explained by their extremely high rates of ownership of urban commercial and industrial property. As of a special census for Buenos Aires in 1909, Italians owned 38 percent of the 28,632 commercial establishments in the city. They were the single largest group of owners of these firms and held double the number owned by native Argentines. All this occurred when they represented only 22 percent of the city's population.²¹ Furthermore, most commentators have stressed that the Italian immigrants participated in industrial activity not only as skilled workers but even more so as entrepreneurs.²² Unfortunately for our purposes, the 1914 census does not break down firm ownership by nationality; it simply provides data for all foreigners as a group as well as for all Argentine nationals. Given the rate of Italian ownership of property above their percentage in the total population, it can be assumed that the Italians held minimally 40 percent of the foreign corporations—their share of the foreign-born urban population—and thus owned at a minimum 26 percent of the 48,779 industrial firms listed in the 1914 census.²³ In fact, they most probably did even better than that, since it is the assumption of all

²⁰ The nature of rural land ownership by immigrants is a much-debated issue in Argentine historiography. Reflecting the traditional viewpoint, James Scobie stressed the inability of the immigrants, especially the Italians, to gain control over the land they farmed; Scobie, *Revolution in the Pampas: A Social History of Argentine Wheat, 1860–1910* (Austin, 1964), 31. Roberto Cortes Conde has challenged this view in the first systematic study of the rural land market for this period. He has shown the existence of a thriving land market in the pampas throughout the period of heavy immigration. Cortes Conde, *El Progreso argentino*, chap. 3. The national statistics, moreover, all support the assertion that Italians were able to become landowners, even able to join the supposedly closed ranks of the cattle ranchers. Although land ownership was not democratically distributed among all groups, Italians did extremely well considering their recent arrival and their initial lack of capital. This same mythology of immigrant inability to purchase the lands they worked has also appeared in Brazilian historiography, where it was assumed that Italians suffered even more discrimination. But recent, systematic studies show that even in the heart of the coffee zones Italians were unusually successful in obtaining commercial agricultural lands; see Holloway, *Immigrants on the Land*, chap. 6; and Thomas W. Merrick and Douglas H. Graham, *Population and Economic Development in Brazil, 1800 to the Present* (Baltimore, 1979), 111–12.

²¹ Alberto B. Martínez, ed., *General Census of the City of Buenos Aires of 1909*, 1 (Buenos Aires, 1910): 130–34; and James R. Scobie, *Buenos Aires: Plaza to Suburb, 1870–1910* (New York, 1974), 260.

²² In the census of 1914, foreign workers accounted for 53 percent of the workforce in commercial establishments and 50 percent of the labor in industrial plants; they accounted for 66 percent of the owners of commercial property and 74 percent of the industrial owners; see Gino Germani, *Política y sociedad en una época de transición* (Buenos Aires, 1962), 195. For a fine analysis of the predominant role of foreign-born residents in the advanced industrial sectors of the nation in 1914, see Gustavo Beyhau et al., "Los Inmigrantes en el sistema ocupacional argentino," in Torcuato Di Tella, ed., *Argentina, sociedad de masas* (Buenos Aires, 1965), 85–124.

²³ República Argentina, *Tercer censo nacional de . . . 1914*, 8: 246. In the industrial census of 1935, foreigners still owned 54 percent of the 50,985 industrial establishments whose owners' nationality was known. And of this total, Italians controlled 41 percent of the foreign-owned firms and thus 22 percent of all such companies. See Oscar Cornblit, "Inmigrantes y empresarios en la política argentina," Instituto Torcuato Di Tella, Doc. de Trabajo, no. 20 (Buenos Aires, 1966), 27.

TABLE 9
Property Ownership by Nationality (as a Percentage) in Argentina, 1914

Nationality	Rural Population			Total Population		
	Relative Importance in Rural Population	Non-Ranch Property Owners	Ranch-Owners (Estancieros)	Relative Importance in Adult Population	Relative Importance among Property Owners	Adults Owning Property
ARGENTINES	77.7	59.4	77.8	51.8	62.2	32.4
ITALIANS	8.7	21.0	6.4	20.2	18.9	25.1
SPANIARDS	6.4	6.4	4.7	16.3	9.7	15.9
RUSSIANS	1.2	3.0	0.6	1.7	0.9	13.9
URUGUAYANS	0.7	0.8	0.9	1.7	1.2	20.6
FRENCHMEN	0.7	2.6	2.4	1.9	2.0	29.5
OTTOMANS	n/a	n/a	n/a	1.2	0.7	16.1
AUSTRO-HUNGARIANS	0.6	2.1	0.8	0.7	0.8	30.5
ENGLISHMEN	0.3	0.4	1.1	0.6	0.4	17.6
GERMANS	0.3	0.6	0.5	0.6	0.4	19.7
SWISS	0.2	0.8	0.8	0.3	0.4	44.2
ALL OTHERS	3.2	2.9	4.0	3.1	1.6	14.3
TOTAL POPULATION IN CATEGORY	3,359,737	72,429	66,561	4,016,739	1,074,964	4,016,739

NOTE: Adult here means more than nineteen years of age. To compensate for the age bias between the native-born and immigrant populations (nationals were on average fifteen years younger than foreigners in 1914), I have taken persons age twenty and above as the basis for my calculations for "Relative Importance in Adult Population" and "Adults Owning Property." In doing so, I had to estimate the adult-age population for four national groups—Russians, Ottomans, Austro-Hungarians, and Swiss—since these were grouped together with "Other Nationals" in the age classifications of the census of 1914. I have used the figure obtained for this general category as the estimator for the four unknown nationalities.

SOURCE: República Argentina, *Tercer censo nacional de ... 1914*, 2: 395–96, 3: 295–309, 4: 68, 5: 837, and 6: 679.

contemporary and later commentators that Italians dominated the manufacturing sector.

Concentrating on rural land ownership and entrepreneurial activity unquestionably yields the best indices of capital accumulation and, ultimately, of social mobility. Further support for the social mobility of Argentina's Italian immigrants could be obtained from data on the occupational distribution of the population; unfortunately, however, the Argentine statistics for this period break down the occupational structure of the skilled work force only by the simple division of foreign and national jobholders for each category. Foreigners in Argentina, who represented only 30 percent of the total population in 1914, dominated almost all of the skilled occupational categories. Among the liberal professions, they were less well represented but usually came out at or below their 30 percent representation in most fields of endeavor. They even represented 35 percent of the capitalists (*rentistas*) of the nation.²⁴

By any standards, these general indices of ownership and occupational distribution attest to the very rapid integration of the resident Italian population into the national economy. Although half of the Italian immigrants returned to Italy, those who chose to remain in Argentina found ready investments for their savings and had little difficulty in achieving rapid economic and social mobility. Italian-born Argentine residents were well represented in every occupation of the nation and were generally over represented in the category of land owners. Even in that most Argentine of all occupations, that of cattle-ranch owner, Italians could be found. Although the available data are obviously not as complete or as comparable as one would like, they all point in the same direction of extraordinary success for a people who had only just entered the national economy and were still overwhelmingly of the first generation.

THE UNITED STATES DID NOT OFFER THE SAME OPPORTUNITIES for Italian immigrants that Argentina did. In studying the mobility of Italians in the United States during the same period, we immediately run into the problem of comparability of data. In the U.S. censuses of this period and even in the special Congressional and Bureaus of the Census and of Labor studies, no relevant figures are provided on land ownership, one of the key variables we were able to analyze in the Argentine case. Moreover, even in the occupational distribution data, there are problems. These national statistical problems, however, are somewhat compensated for by the existence of numerous samples of the population, which have been studied both by U.S. government agencies and by later social scientists. Thus, samples of families nationally and of male workers in large urban centers on a local level will provide much of the basis for the following discussion.

What these disparate sources reveal is that, in the first decade of the twentieth century, the "New Immigrants" were consistently in the lowest paying and lowest status jobs in the United States. Moreover, when the New Immigrant category is

²⁴ República Argentina, *Tercer censo nacional de ... 1914*, 4: 382-97.

broken down into nationalities, it was found that Italians were consistently at the lowest end of the scale. Although some economic historians have recently argued that this wage and status differential was due to lower skill levels, lack of English, and short-term length of residence in the United States, statistics concerning second-generation Italians—that is, children of Italian-born parents—also reveal extremely low rates of social mobility for these persons of Italian descent who are literate and have lived their entire lives in the United States.

What both the Dillingham Immigration Commission *Reports* of 1909 and the Labor Commissioner's survey of 1903 reveal is that Italians consistently were at the bottom in family income and of male and even female income in contrast to all other foreign-born workers. In each case they were lower than two-thirds of the New Immigrants and in most cases were at the very bottom of the scale. Even home ownership, sometimes a heavy investment for immigrants in the lower ends of the occupational scale, was a distinctly minor factor among the Italians. Where the Italians showed a positive feature was in their capacity to save. Italians, if this very small 1903 national sample is to be accepted, were probably among the very best savers among immigrant groups. (See Table 10.) This characteristic, which we will return to in later discussions, tells us a great deal about Italian attitudes toward savings and toward repatriation to Italy. It also reveals an unwillingness to sacrifice repatriation of savings at the cost of local investments.

A well-drawn sample of Italians in New York City from the heart of the Italian districts at the beginning of this century reveals the same patterns found in the national surveys. Comparing occupational stratification between the Italians and the Russian Jews, who arrived at the same time as the Italians to the United States, reveals just how different the Italian mobility experience had become. The Russian Jews were, of course, at the other extreme from the Italians in terms of repatriation, being the classic case of the immigrants who could not return to Europe and were therefore concerned most completely with "making it" in America. By 1905 in New York City, the Russian Jews already had reduced their numbers in unskilled and semi-skilled occupations to 19 percent as contrasted to 58 percent of the Italians who remained in these occupational categories (see Table 11). Obviously, this is an extreme comparison, and, in contrast to the Polish immigrants, the Italians would not appear in such an unfavorable light. Nevertheless, it does reveal just how skewed the occupational distribution of these Italians were and why they were consistently coming in with the lowest incomes.

The question to be asked in this context is why the Italians consistently had the lowest rates of income, ownership, and job skills. Clearly, there was extreme prejudice against Italians in the United States, especially those considered to be from southern Italy. The *Reports* of the Immigration (or Dillingham) Commission of 1910 are especially flagrant in the racist attacks on southern Italians. But more important than the discrimination, which tended to exist in every American society that received foreign immigrants, were the preferences of the Italians themselves and the nature of the labor market they entered.

Italians deliberately concentrated their activity on insecure and unskilled laboring jobs, which had short-term potential for high savings. They also tended to

TABLE 10
Comparative Economic Status of Immigrant Workers
in the United States, 1903 and 1909

	<i>Italians</i>		<i>All Foreign Born</i>	
SURVEY OF 1903				
Mean Annual Family Income	\$611	(256)	\$711	(10,279)
Percentage of Homeowning Families	12%		24%	
Percentage of Savings (Income/Expenses)	7%		4%	
SURVEY OF 1909				
Mean Annual Family Income	\$595	(1,963)	\$704	(13,825)
Mean Annual Income of Males (18+)	\$417	(3,553)	\$455	(22,938)
Mean Annual Income of Females (18+)	\$235	(320)	\$284	(2,386)
Percentage of Homeowning Families	19%	(2,258)	22%	(15,551)

SOURCES: For the 1903 survey, see U.S. Commissioner of Labor, *Eighteenth Annual Report, 1903* (Washington, 1904), *passim*; and, for the 1909 survey, see U.S. Immigration Commission, *Reports*, 1: 407–12, 468.

concentrate in those regions and cities where such jobs were most abundant and tended to shun the states of the Middle West, for example, where the industrial core of the nation was developing, and the South, where their market was restricted by a native black and white labor force in abundance. The limited availability of farm occupations, which grew at a slower rate than the rest of the labor force, also offered few attractions. In contrast, construction, railroad, manufacturing, and trade jobs were increasing faster than the rate of increase of the labor force.²⁵ Thus Italians concentrated in all of these more expansive occupations, except manufacturing. In the United States as in Argentina this strategy paid off. With average daily wages of laborers varying from \$1.25 to \$1.50 per day, and with steamship fares costing on average \$15 to \$20 per person for steerage class from Europe to the United States, it is no wonder that the Italian response to the U.S. labor market was so elastic.²⁶ Over time, moreover, real wages in the United States, as in Argentina, were on the rise for most of the period from 1880 to the first decade of the twentieth century.²⁷

²⁵ U.S. Bureau of the Census, *Historical Statistics of the United States*, 139, 144–45.

²⁶ Gould, "European Inter-Continental Emigration, 1815–1914," 611–15; Philip Taylor, *The Distant Magnet: European Emigration to the U.S.A.* (New York, 1971), chap. 8; and Sori, *L'Emigrazione italiana*, chap. 8.

²⁷ Between 1890 and 1914 real wages, both hourly and daily, increased by over 30 percent in manufacturing, and from 1880 to 1914 real wages rose by 48 percent for all nonfarm labor; Albert Reese, *Real Wages in Manufacturing, 1890–1914*, National Bureau of Economic Research (Princeton, 1961), 120; and U.S. Bureau of the Census, *Historical Statistics of the United States*, 164–65.

Thus the response of Italians to U.S. labor market conditions helps explain their relative concentration in certain low-status, economically remunerative jobs. But, in their very response to that market, the half of the migrants who decided to remain and construct a new community found themselves, through the result of their timing and concentration, in a poor position to compete with both other immigrant groups and native-born Americans. The concentration of Italians in the urban centers of the Northeastern states of the United States was a negative influence on their future rates of mobility. As a recent survey of occupational mobility and location concludes, "Opportunity for migrants in the nineteenth century was greatest in young, small communities outside the Northeast."²⁸ This locational disadvantage still influenced social mobility into the first decade of the new century

TABLE 11
Occupational Distribution of Immigrant Heads of Households
in New York City, 1905

<i>Occupational Category</i>	<i>Percentage of Italians</i>	<i>Percentage of Russian Jews</i>
HIGH WHITE COLLAR	2	15
LOW WHITE COLLAR	18	31
SKILLED	22	35
SEMI-SKILLED	16	18
UNSKILLED	42	2
TOTAL IMMIGRANT HEADS OF HOUSEHOLDS	1,015	963

SOURCE: Thomas Kessner, *The Golden Door: Italian and Jewish Immigrant Jewish Mobility in New York City, 1880–1915* (New York, 1977), 52, 60.

as well. The decision of Italians therefore to concentrate in the oldest regions (72 percent lived in the Northeastern states in 1910) and primarily in urban areas (78 percent) in the long run had a negative impact on their potential social and economic mobility.²⁹ That this decision to concentrate in the urban Northeast was a reasonable one in terms of earning potential for unskilled laborers in turn became a

²⁸ Kirk and Kirk, "The Immigrant," 231–32.

²⁹ By 1920, 84 percent of the Italian-born residents of the United States lived in urban areas, and 73 percent lived in the Northeast. This was the highest concentration in this region for any immigrant group. When first- and second-generation Italians are added together, their concentration in the Northeast in 1920 rises to 81 percent. Niles Carpenter, *Immigrants and Their Children, 1920* (Washington, 1927), 368, 372. On the occupational distribution of Italians and their relatively limited role in U.S. agriculture, especially in contrast to their much larger role in Argentine and Brazilian agriculture, see Foerster, *The Italian Emigration of Our Times*.

negative factor in their possibility for advancement once a permanent community had been established. This in turn helps explain the relative low rates of mobility that we have seen for the Italians compared to other New Immigrant groups. Thus, the relative importance of the Italians within the labor force, their late arrival in North America, and their consequent concentration in low-status, unskilled jobs tended in the aggregate to make the mobility of Italians in the United States quite different from their experience in Argentina.

When Italians finally settled, they found high-status jobs blocked to them, they found agricultural lands in the hands of native-born Americans or other immigrants, and they found most of the industrial establishment fully in the control of native-born Americans and rapidly moving toward monopoly situations. This does not mean that there were not pockets of rapid mobility for some Italians. The 9 percent of the Italians who were listed as living in the West in 1910 clearly had much in common with the Italians who went to Argentina. Arriving early in the settlement process, they quickly entered into farming and wine production and soon had rates of mobility that were probably quite comparable to those in Argentina. But for the majority of Italians their concentration of settlement in the older regions of the United States meant that long-term mobility, even for the second generation, was at a much lower level than that experienced by other immigrant groups of both the New and Old migrations. Italians did not do well comparatively in terms of occupational, educational, or social mobility.

An analysis of the U.S. census of 1950, for example, shows that first- and second-generation Italians had the highest percentage of unskilled, nonfarm laborers and the lowest percentage of professionals of all major immigrant groups. Only the Poles had fewer white-collar workers, and only the Irish had fewer in the skilled craftsmen category. (See Table 12.) This pattern of lower mobility among Italians is even more evident in a detailed study in 1950 of first- and second-generation immigrants in Boston, a major center of Italian migrants. Italians were the least well off of all major ethnic groups in the city, having on average the least schooling, the fewest high-status white-collar workers, and the lowest income.³⁰

Thus, even as late as a generation following the end of massive immigration from Italy, Italians had not achieved a significant rate of mobility in the United States, compared not only to native white North Americans but even to all other immigrant groups. Yet, by 1920, almost half of the so-called Italian *stock*, as defined by the census, was born in the United States, a figure that rose to over two-thirds of those of Italian origin in the United States by 1950. Clearly, the Italians in Argentina had achieved a position in the professions, in land ownership, and in industry by 1914 not achieved by their contemporary migrants to the United States by 1950.

³⁰ Stephan Thernstrom, *The Other Bostonians: Poverty and Progress in the American Metropolis, 1880-1970* (Cambridge, Mass., 1973), 172. Even as late as 1960, a U.S. Bureau of the Census survey of second-generation males shows that, of all the so-called New Immigrants from Southern and Eastern Europe, Italian Americans had the lowest percentage of professionals among all age groups, including the youngest (those from twenty-five to thirty-four years of age); Stanley Lieberson, *A Piece of the Pie: Blacks and White Immigrants since 1880* (Berkeley and Los Angeles, 1980), 330.

TABLE 12
Comparative Nonfarm Occupational Structure (as a Percentage)
for the Major Immigrant Groups in the United States, 1950

<i>Occupation of Employed Males</i>	<i>Native American Whites</i>	<i>Germans</i>	<i>Italians</i>	<i>Poles</i>	<i>Irish</i>
PROFESSIONALS	9	9	6	7	10
MANAGERS AND SUPERVISORS	12	15	11	9	12
CRAFTSMEN AND FOREMEN	23	28	22	23	20
OPERATIVES	24	20	30	33	18
CLERICAL, SALES, AND SERVICE	22	22	22	19	33
DAY LABORERS	8	6	9	8	6
TOTAL NUMBER OF EMPLOYED MALE IMMIGRANTS	21,217,560	1,096,620	1,118,940	721,710	569,370

NOTE: Given the high concentration of native whites and the extremely low participation of most immigrants in agriculture, I decided to show only nonfarm labor (for which all occupational unknowns have been deleted).

SOURCE: E. P. Hutchinson, *Immigrants and Their Children, 1850-1950* (New York, 1956), 335-49.

THIS VERY PRELIMINARY SURVEY OF COMPARATIVE ITALIAN IMMIGRATION reveals several important themes. The Italians who migrated to Argentina and the United States were concerned with accumulating savings through the higher wages available in the Americas. In most cases, the immigrants assumed that those savings would be invested back in Italy either in land or other economic activities so as to improve their economic and social standing.

In the Argentine situation the relative economic opportunities were such that many immigrants were attracted to invest their savings in the local economy. Skilled artisans and professionals, moreover, were more drawn to the Argentine situation because of the tremendous expansion of both agriculture and industry. Since the Italians were the premier immigrant group and in fact accounted for some 14 percent of the national population, their potential for investment of their savings in America was extraordinary. The fact that so many still returned to Italy is testimony to the tremendous commitment of most immigrants to return to the mother country.

Overall, the same factors of wage differentials and high savings potential pulled Italians in even larger numbers to the United States. Here the opportunities for unskilled labor were extraordinary, although jobs in heavy industry and agriculture were far less available than they were in the Argentine. But the labor market for low-status, unskilled workers was expanding so rapidly that the Italians found even their competition with other immigrant groups no impediment to a rapid accumulation of savings. That the majority of them returned to Italy is testimony to their successful analysis of the market conditions open to them. But, for those who stayed, the relative closure of higher-status jobs and the earlier strategies that had

stressed current income and repatriation of savings over local investment in land or education reduced occupational mobility. Second-generation Italians were still more heavily concentrated in unskilled and manual labor jobs than were most other major immigrant groups. Although third-generation Italian-Americans were becoming indistinguishable from native-born North Americans, the process of assimilation took much longer in the United States than it did in Argentina. Most importantly, it was the relative structure of the individual labor markets of the host countries that determined the patterns of integration and mobility of the immigrants who remained. No significant factors in the Italian origin of the immigrants, or in their cultural make-up, can as fully explain the social and economic history of the Italians in the Americas.

Comments:

HERBERT S. KLEIN HAS WRITTEN A VERY STIMULATING ESSAY on the comparative history of Italian immigration to the United States and Argentina. I believe migration scholars owe him a great debt for his effort to put together sound theory and available information to illuminate one of the most relevant, recurrent issues in migration research—namely, the economic integration of migrants in the host society. Since I am convinced that his essay deserves to become very influential in the years to come, and should in fact inspire further work in the comparative history of immigration, I will be quite critical in my comments, despite my sympathy with his approach. As a sociologist, I have found some serious conceptual and theoretical difficulties in his argument. Therefore, I will focus on them, while addressing myself to empirical issues only when they are part of those difficulties.

But another critique comes from my own expectations about what a skillful social historian like Professor Klein should do—namely, tell us something convincing enough about the human experience of the groups he is writing about. I missed an even elementary description of the world these immigrants faced, some feel for the chronology of events, the maturation and change in opportunities, the growth of communities, the changing conditions faced by innovators and mass followers. About all of these points, I can only voice an unfulfilled expectation.

THE MAIN ARGUMENT IS SUMMARIZED IN THE FINAL PARAGRAPH: “it was the relative structure of the individual labor markets of the host countries that determined the patterns of integration and mobility of the immigrants who remained. No significant factors in the Italian origin of the immigrants, or in their cultural make-up, can as fully explain the social and economic history of the Italians in the Americas” (page 229). There are four pitfalls in the development of this argument in Professor Klein’s essay: (1) some confusion about what is to be explained, that is, integration, mobility, or the two combined (are both one and the same thing?); (2) assuming that integration into the local society is the key issue, lack of reference to the overall stratification system; (3) overemphasis on the two migratory streams almost solely as a function of a fit to individual labor markets by potential migrants, with a concomitant underemphasis on all other differences—like those in origins—as explanatory variables; and (4) insufficient attention to return migration.

To start with the first of these, the “sharp differences in the Italian immigrant experience” (page 306), which Professor Klein hopes to explain, are never clearly

defined. The title refers to "integration," qualified in the text as "economic." He also refers to "integration in terms of [the] occupational distribution, social mobility, and relative wealth" of the immigrants (page 306), and to "the relative rates of mobility and integration within the two societies" (page 320). There are, in my opinion, three distinct, legitimate comparisons between Italian immigrants in the United States and Argentina: (1) a comparison of the status of Italians relative to non-Italians within the stratification scale of Argentina and the United States; (2) a comparison of the differences between status of origin and destination among Italians in both countries; and (3) a comparison of the standard of living of Italians in Argentina and the United States. The first comparison, in what might be called "integration," is apparently what has attracted Klein's attention in this essay. It is also the most difficult one, because it necessarily involves a discussion of origin and mobility of Italians for both destinations as well as the location of other groups.

The second comparison, in what I term "mobility," is also very complex because it demands an analysis of relative opportunities of Italians in both societies, but not necessarily relative to non-Italians. My impression is that Professor Klein consistently dumps the first two comparisons together and thereby adds some confusion to what is admittedly a difficult set of problems for empirical research. Furthermore, I notice the absence of the third comparison, not to blame the author for what he has not done but simply to point out a most relevant issue, today, for us, as it probably was for Italians in both countries and in Italy.

Why Italians had, on average, a higher relative status in Argentina than they did in the United States, apparently the main issue in Professor Klein's essay, may actually have been determined by labor markets that resulted in greater opportunities for advancement in the South American country. But there is no way by which relative status can be studied without consideration for the status of others—Klein's second pitfall. Klein, of course, offers a number of suggestions as to this difference in the experience of Italians, mainly in the relative impact and timing of their immigration. He does not, however, seem to go far enough in this regard. My point is that the little education, training, and probably tangible capital Italians brought were scarcer in Argentina than in the United States, not merely that they were bringing more with them to Argentina (see my next point). Italians arriving in Argentina found positive rather than negative prejudice, being favored over natives as tenant farmers and as urban workers. Italian agricultural laborers did not have to adapt to cattle ranching, since so few of them were needed for the latter, while native laborers did have to adapt to intensive farming, unlike Italians. Although not highly skilled on the average, Italians had more industrial experience and as much literacy as the average non-Italian laborer in Argentina. In sum, a comparison between Italians and non-Italians is missing for both countries.

Comparisons in relative status are made more difficult by the fact that Argentine statistics are better for examining proportional property ownership, whereas U.S. data are more plentiful for ascertaining relative occupation and income distribution. Professor Klein rightly points out, following Roberto Cortes Conde and rejecting the traditional stereotype, that the agricultural land market was open and that Italians often became landed farmers. Marketing and financial control over

farmers, however, might well have erased much of the advantage derived from property. And, in any case, the data on rural and urban ownership should be compared to population data only with better control for the age and sex composition of Italian immigrants, since adult males are obviously overrepresented among both owners and immigrants. Klein does control for age (but not sex) in Table 9 (page 322) but does not control for either in discussing urban commercial and industrial property in Buenos Aires (page 321). All that this evidence can tell us is that productive property (urban *and* rural) was a possibility for Italian immigrants rather soon after they arrived—that there were no institutional impediments to acquiring it. How did they gain terrain in this area? To what extent (compared to other groups) did they obtain it? And what effects did ownership have on their social mobility? All of these questions remain to be explored.

How different, and why, were Italians arriving in Argentina from those reaching the United States? This is the third pitfall, and Professor Klein's research strategy is slightly confusing on this point. It is well known that a crucial difference lay in the north-south composition of both streams. But, after showing this difference, Klein argues that the north was not so different from the south, presumably to indicate that cultural and other variations did not count for much in explaining the divergent experiences in America. Literacy *was*, however, a crucial difference: by 1901 two out of every three southerners were illiterate, while only one out of every three northerners was. Among migrants to the United States, 12 percent of the northerners were illiterate, versus 54 percent of the others, for a total illiteracy of 47 percent. The breakdown is not available for Argentina, but the roughly comparable figure for all Italian immigrants is 36 percent. All of this information is provided by Klein's tables: Italians arriving in Argentina were, on the average, better educated than those arriving in the United States. The fact that more northern Italians migrated to Argentina should have explained this difference, although from both regions some positive selectivity was probably involved.

The occupational comparison among arriving immigrants, on the basis of immigration statistics, serves little purpose, reflecting as it does the criteria of immigration officials in the two countries. After a painful discussion of the data in Tables 5, 6, and 7 (pages 311–15; for the tables, see pages 313, 314, 316), Professor Klein acknowledges that "the entire distinction between nonfarm unskilled laborers and farm workers may have been rather artificial" (page 314). I fully agree. More *interesting is the difference in sex composition. Klein's data reveal much higher sex ratios among Italian migrants to the United States than among those who went to Argentina—as shown by immigration statistics but not by census data. This should indicate a larger component of males and heavier return migration from the United States. But more on repatriation later.*

Then, if these differences in origin exist, how do we explain them? Professor Klein, following Michael Piore's thesis, chooses to explain them by reference to the contrast in labor market structures: in the United States there was by the end of the nineteenth century a greater demand for unskilled, temporary, urban workers to fill what were essentially high-wage (by Italian standards), dead-end jobs. In Argentina, however, this demand was not so marked, while there were greater

opportunities in skilled, career-oriented jobs, even if comparable wages were slightly lower. This argument implies a fit between migrant selectivity and labor-market conditions. It can be rephrased as follows: Assume there are two types of potential migrants in Italy, Type A and Type B. Type A is made up of those who want to find a job and earn high wages with the idea of saving money and returning home. Ideally, young, single, and illiterate males fit this description. Type B is made up of those who consider a career, rather than a job, with the idea of settling where such a career is possible, with no serious prospect of returning home for some time. Not-so-young males, with their families (sometimes with a split involved), some skills, and perhaps some cash collected from selling any property available, fit better here. The opportunity structure attracted Type A migrants to the United States and Type B migrants to Argentina.

This explanation is appealing to me, and the data at hand seem congruent with it. But, curiously enough, Professor Klein dismisses the most obvious historical explanation of these differences in migrant origin: migration from Italy to Argentina became quantitatively more important much earlier than it did to the United States. There was an important Italian community in Argentina, and mainly in Buenos Aires, even before reunification. In 1869 Italians made up 23 percent of the population of Buenos Aires. Most of them, of course, came from northern Italy, since it was the north that was more closely linked to the world economy. These Italians—for instance, those from Genoa—became very important in trade, transportation, and crafts. They provided a crucial intermediation in recruiting farmers when the agricultural revolution in the Pampas began as well as in bringing in urban workers. At that time, however, there was in the United States no Italian community to speak of. It is possible, but irrelevant here, to explain why migration to Argentina started as early as the 1850s and only in the 1880s to the United States. But once it began, and a preponderant northern Italian presence existed in Buenos Aires, immigration followed as usual the informal, locally based network for the recruitment of potential immigrants. In the north there was a much greater availability of Type B potential migrants than in the south. The demand for unskilled temporary workers in Argentine agriculture from 1875 to 1895, I would guess, was also supplied predominantly by northern migrants, but there was no reason why this same pattern had to apply to the United States in the following decade.

The most troublesome of the many issues involved in this fascinating comparative history—and the fourth pitfall—is return migration. Overall available figures show a slightly higher rate for the United States than for Argentina, some 54 percent as against 51 percent. Yet the entire reasoning based upon the different labor markets would lead us to believe that repatriation should have been much higher among Italian migrants to the United States. There is partial evidence for this in the contrast between the sex ratios among incoming Italians and immigrants registered by the census (366.5 versus 190.6 in the United States, and 267.8 versus 171.2 in Argentina; Tables 7 and 8, pages 316, 317). I am tempted to believe that the Argentine estimate of returnees is too high, but I have no way of showing it. It should be noted in passing that crossings to and from Uruguay and Brazil resulted,

mainly for Italians, in many moves that are not included in the official immigration statistics.

The incidence of return migration is, of course, important as an indicator of differential selectivity according to labor market structures. But it is also crucial to any explanation of the relative achievement of Italians in America. Professor Klein implicitly recognizes this issue in the apparently contradictory way he handles repatriation from each host country. About Argentina he tells us, "the fact that so many still returned to Italy is testimony to the tremendous commitment of most immigrants to return to the mother country" (page 328). About the United States, however, he says, "That the majority of [the immigrants] returned to Italy is testimony to their successful analysis of the market conditions open to them" (page 328). In other words, returnees are sometimes a failure and at other times a success. This is not stretching the notion to fit our taste but, rather, a consequence of the assumption about two types of migrants. The net effect, it should be noted, is that those remaining *must* more often be relative failures in the United States than they were in Argentina due to the selectivity of return migration. In fact, this should be more so if, given a different type of composition for the two migrant streams, we find approximately similar rates of return migration: Type A migrants remaining in the United States "by mistake" had few chances of moving up, while Type B migrants returning from Argentina "by mistake" would have had no effect upon those remaining.

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HERBERT S. KLEIN HAS WRITTEN a valuable and interesting essay that makes a significant addition to understanding pre-1914 international migration. In regard to two important aspects of his analysis, I find myself applauding very warmly.

The first of these is the substantive findings regarding the integration of Italian immigrants into the economies of the United States and Argentina. Here I find Professor Klein's argument not only convincing but welcome, since in one of my recent essays on intercontinental emigration I hazarded the statement that it appeared "on a cursory view extremely probable that the Italian immigrant had a better chance of upward movement in Buenos Aires than in New York." I had to couple with this statement the admission that it was impressionistically based and that I was unaware of any studies of occupational mobility that would rigorously validate it. By his innovative presentation of comparative material on occupational structures and property ownership patterns of Italian immigrants in the two countries, Klein has provided hard evidence to buttress my view, and I suppose

some personal satisfaction at finding one's own hunch confirmed adds to the approval with which I view this aspect of Klein's work.

The second praiseworthy feature is that Professor Klein's approach constitutes a very rare example of readiness on the part of a U.S. historian to recognize the element of competition between the United States and other countries of immigration. European historians, and I include myself in this category, have repeatedly complained that American contributions to the literature of migration studies, both of the econometric and of the traditional variety, have been cursed by an overly "U.S.-centric" bias and have too often analyzed the issues as though the United States were the only country of immigration, as though its links with each sending country could be considered separately and in isolation. In fact, patterns of migration between various European countries and the United States must in each case be considered in the context of a far more complicated pattern of movement, recognizing that there were a number of (competing) receiving countries, many sending countries, and an environment of generally heightened mobility in which *intra*-European migration played a very important part. Klein's essay goes at least some way toward abandoning that U.S.-centric vision of the process of migration, a vision I find distorting and undesirable.

AGREEMENT CANNOT, HOWEVER, BE COMPLETE. Indeed, it is easy to see that something *must* be wrong with Professor Klein's thesis, since its assumptions and hypotheses lead to a conclusion at variance with the facts. Consider: if northern and southern Italy were really fairly similar, as Klein asserts, in terms of their socioeconomic characteristics, and if the composition of the emigrant stream were determined only by the strength and nature of the demand for labor emanating from the various receiving countries, then one should expect no appreciable difference between these Italian regions in regard to the respective sizes of their emigrant cohorts and the destinations to which they moved. The three major countries of immigration in the New World, that is to say, would draw forth streams of migration differing in size and composition as determined by the respective strengths and natures of the labor demands they exerted; *but from each region of Italy* the percentage distribution of destinations would be similar, as would the size of their emigrant flows when measured *per mille* of their own populations.

We know that this is not in agreement with the facts. On the contrary, while by the early twentieth century the major regions of Italy supplied emigrants in numbers representing fairly similar proportions of their own population, this had not usually been the case; for most of the later nineteenth century there were large differences in emigration levels from northern and southern Italy and still more from insular Italy. In addition, emigrants from southern Italy always had a strong preference for the United States over Argentina and Brazil, and migrants from the north had a somewhat less-pronounced preference for Latin American countries. While these regional destination preferences were in the process of being reduced in the pre-1914 decades, as Professor Klein rightly notes, they had far from disappeared on the eve of the First World War.

Clearly something is wrong, and two logical possibilities present themselves. Either Professor Klein has underestimated regional socioeconomic differences within Italy, or there were determinants of the size, composition, and destinations of the emigrant stream other than the labor demand exerted by the countries of immigration. In fact, I believe both of these are true. As to the first, I am no specialist in Italian social or economic history and can only comment that, in my view, the differences revealed by Klein's Table 4 (page 312) in respect of such indicators as vital statistics, per capita incomes, and illiteracy are quite large enough to have had a major impact on the response to opportunities for emigration. Klein himself belittles these differences by observing that they are much smaller than those to be found between the modern and traditional sectors of many Third World countries today (page 311). Without doubt this is true; but saying so applies an irrelevant and far too demanding test. One need only consider the enormous difference in patterns of emigration between, say, the France and the Germany of the 1850s to 1880s to realize that far smaller socioeconomic contrasts than those between the traditional and modern sectors of a present-day less developed country could generate huge differences in migration propensities. Further, Klein fails to take into account the employment opportunities offered by northern, southern, and insular Italy, which both differed among the regions and changed over time.

This observation leads to the second point, which is that Professor Klein concludes far too readily that conditions in the *receiving* countries dominated migration patterns. It is true that this seems to be suggested by many other studies, including some of the recent econometric investigations of migration. My contention, however, is that what these studies successfully explain is the *fluctuation over time* in the volume of migration rather than its fundamental determinants and motivations. A pervasive and highly important influence on migration, moreover, was "feedback"—relaying back to friends and relatives in Europe information, encouragement, and financial help, which generated geographical and occupational preferences far more pronounced and enduring than objective socioeconomic determinants alone would warrant.

Finally, intercontinental migration must be set against the background of increased mobility *within Europe* itself—and certainly not least in the case of Italian emigration. Indeed, I frankly find it astonishing that a study of Italian emigration patterns totally ignores the massive annual movement of workers from northern Italy to other European countries, for this was the most important single determinant of the size and composition of the migrant stream from northern Italy to transatlantic destinations. If northern Italy, that is to say, did not provide transatlantic migrants looking for unskilled or semi-skilled work in the industrial and service sectors of American cities in anything like the same degree that southern Italy did, that was overwhelmingly because northern Italian workers seeking such employment traditionally found it by seasonal migration to neighboring European countries. Even on the official Italian statistics, the annual outflow of such workers to European countries generally exceeded that to all transatlantic destinations combined until the early twentieth century, and we have known ever since the writings of Coletti more than seventy years ago that the Italian statistics of seasonal emigration to Europe grossly understate the true magnitude of the

movement. Thus, regretfully, we have to conclude that even in Professor Klein's case the liberation of American historiography from its "U.S.-centric" bias is far from complete.

FINALLY, I AM SOMEWHAT PUZZLED by the last two paragraphs of Professor Klein's essay. Though presented to the readers as a summary of his argument, these paragraphs, if I interpret them aright, in fact introduce a novel and more far-reaching hypothesis—namely, that "in most cases" Italian emigrants *both* to the United States and to Argentina intended, on departure, subsequently to return to Italy (page 328). Whether or not one can agree with this hypothesis doubtless depends on *exactly* what "in most cases" means. A few comments are, in any event, in order.

In the first place, we cannot, of course, perceive emigrants' motives or intentions directly, except perhaps through occasional biographical or autobiographical studies; we can only draw inferences about motive and intent from summary statistical data. Viewing that evidence, it may certainly be surmised that *many* Italian emigrants never intended to stay permanently in the countries in which they sought work; and it is also probable that a substantial number who did intend to stay permanently in fact changed their minds and returned to Italy after a period overseas. But the same evidence tells against the view that all or even the large majority intended only a temporary stay abroad. Data on the sex and age distribution of the emigrant cohorts, for example, permit some inferences about the extent to which the various emigrant nationalities intended their stay abroad to be permanent or temporary, for we know that migration *intended to be temporary* is predominantly young adult and male, whereas migration *intended to be permanent* is more evenly balanced between the sexes and age groups. Such evidence suggests that Italians were indeed *toward*, though not *at*, the "temporary" end of the spectrum with regard to their intentions as well as in the event; but the same evidence is very far from suggesting that all, or even a large majority, intended only a temporary stay in the New World. Further, it is almost certainly true, as much for Italian immigrants as for those of other nationalities and times, that the number who originally intended to stay permanently but in fact returned within a few years greatly exceeded the number who intended a temporary sojourn abroad but in the event decided to stay permanently; if so, intentions were revised, on balance, in the *opposite* direction from what Professor Klein seems to suggest.

The eighteen-hundred-word limit imposed on me by the editors does not permit an extended discussion of this and similar points of contention. At the risk of meriting an accusation of bad form and self-advertising, I cannot do better than refer the reader interested in this and other points at issue to the series of essays that I recently published in another journal—particularly to the third, fourth, and fifth of those essays.¹ These essays not merely develop more fully some of the views

¹ These five essays are to be found in the Winter 1979 and Spring and Fall 1980 issues of the *Journal of European Economic History*. Especially see "Return Migration from the U.S.A.," *ibid.*, 9 (1980): 41–74, "Some Features of Italian Emigration and Emigration Statistics, 1876–1914," *ibid.*, 74–112, and "The Role of 'Diffusion' and 'Feedback,'" *ibid.*, 267–315.

expressed in this “comment” but also reveal to the careful reader other points of both agreement and disagreement between Professor Klein and myself that I have not had space to refer to here.

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THE DISCUSSION OF WHY, whether one looks at the labor movement, the professions, or the land, Italians did so much better in Argentina than they did in the United States has progressed haphazardly for quite a while. It is fortunate that Herbert S. Klein has decided to face the issue frontally. He refuses to attach decisive weight to the regional origin of Italian immigrants (overwhelmingly southern in the United States, with a slight majority of northerners in Argentina). Not all of his arguments are equally convincing: admittedly, for demographic characteristics and distribution of the economically active population, northern and southern data are roughly similar. As Klein knows better than I, however, demographic patterns reflect more general social change only after a time lag, and his occupational categories are so extremely aggregated that they are not particularly illuminating. Even so, it is difficult not to wonder whether, even in 1900, “industrial worker” meant the same in northern and southern Italy.

Part of the problem is, no doubt, that figures are not as abundant and as relevant to his questions as Professor Klein would wish them to be. The problem becomes more acute when he tries to assess how well Italians really did in Argentina. The contrast with the quality and quantity of information available for the United States is painfully evident, and, for lack of better indicators, enormous weight is given to figures of Italian urban and rural property ownership.

This is perhaps unavoidable, but also dangerous. Although Roberto Cortes Conde’s admirable *El Progreso argentino* (1979) has fully documented the existence of a thriving market in rural and urban property (and, it is to be hoped, put to rest the peculiar notion, mindlessly echoed by so many otherwise shrewd scholars, that the traditional landed class held an effective monopoly on rural real estate), the workings of such a market, as Klein rightly reminds us, did not necessarily bring about a democratic distribution of ownership. Even the uninformative 1914 census can yield some indications of the consequences of this far from complete equality. It is, for instance, suggestive that, while in the city of Buenos Aires as a whole Italians owned 32 percent of all urban properties, the percentage dropped to 14 percent in *circunscripción* 12 (the older city center) and 13 percent in *circunscripciones* 13 (the new downtown area) and 20 (the upper-class residential quarter); but 41 percent of all real estate owners were Italian in *circunscripción* 1, an immense swampy district of

recent lower-class settlements.¹ These figures reflect both the very real achievements of Italian immigrants and the severe limitations.

The same holds true for the countryside, but here the risks are perhaps higher: after all, 87 percent of all rural properties were less than 500 hectares in size,² and, except for the wine districts, this upper limit was far less than could give anybody a claim to an even modest place among the landed classes. Global figures on the numbers of landowners are, then, extremely uninformative. Here again, the census might yield more relevant—albeit indirect—information if its immense wealth of local data were put to use.

What is immediately available does not completely support Professor Klein's contention that "a remarkably large proportion of Italians succeeded in owning land," even in the grain-growing areas, where they were "especially successful" (page 321). In Santa Fe—where Italians had entered agriculture earlier and in larger numbers than in other provinces—only 20 percent of the Italian farmers tilled their own land; this figure is lower than the percentage for other foreign farmers (28 percent), and much lower than that for native Argentines (43 percent). Admittedly, these figures are partially misleading, in that they stress the difficulties of Italian immigrants in the twentieth century, during a sustained boom in land prices, and partially hide the success of earlier waves of immigration, some of whose offspring were, of course, to be found among the native landowning farmers. Even so, the last group is too small—less than 9 percent of the total—substantially to modify the unwelcoming social landscape of the grain-growing district where Italians had been most successful.

This is only to say that Professor Klein's conclusions are tentative, as they must be after what is, after all, only a "very preliminary survey" (page 306), and he is to be congratulated for having offered a pertinent set of questions that bring the discussion into focus. He has also done something more important: by stressing timing—that is, the position in the sequence of migratory waves of each ethnic immigrant group—he has suggested a valid new direction for further studies on the subject.

HERE, I GUESS, IT WOULD BE WISE to stop and bow out, after offering the usual hopes that further data will answer the still extant questions. Let me, however, be less wise and try to place the comparative exploration to which Professor Klein invites us in the context of Argentine historiography and its desirable future developments. I shall try to do this by putting together a view of the Italian role in Argentina that can be obtained from the historiography—an extremely sketchy one, I am sorry to say—and the questions this view poses (and more frequently begs), questions that a comparative approach may help answer.

From this admittedly self-serving viewpoint, what I miss most in Professor Klein's presentation is the awareness that—compared to the United States—Italians not

¹ República Argentina, *Tercer censo nacional de . . . 1914*, 10 vols. (Buenos Aires, 1916), 4: 3–6.

² *Ibid.*, 5: 837–00.

only had a very different place in the sequence of external migrations to Argentina but also went to a vastly different country. True, by 1914, not a few Argentines saw the differences as unessential, and this assessment, though hardly prophetic, was not purely wishful thinking. Three-quarters of a century earlier, it would have been pure folly; already a first configuration of the Italian community and its place in Argentina was taking place.

When Rosas fell in 1852, probably the majority of the economically active population in the city of Buenos Aires was already foreign born, and foreigners (especially the Irish and Basque immigrants) were settling in large numbers in the countryside. Some of these, as well as numerous groups of retail traders, craftsmen, and the like in the city, were thriving as a privileged sector within the new middle groups that were quickly changing the social physiognomy of the region. Among them, the Italians—overwhelmingly Genoese in origin—were prospering in river navigation and trade and were cornering the produce supply to Buenos Aires; a Genoese neighborhood was growing in La Boca, the Buenos Aires harbor for fluvial navigation.³ One of these Genoese merchants, Giacinto Caprile, worth one million paper pesos (around £15,000) and by no means the wealthiest in the group, managed through personal links with the minister of foreign affairs to have his brother-in-law appointed Rosas's consul in Genoa (much to the dismay of the Sardinian representative, who rightly suspected him to be a supporter of Mazzini's *Giovane Italia*).⁴ He, like other prosperous Italians, had acquired the status of a respected, if not leading, member of the foreign mercantile community, a community whose position at the top of local society was deplored by not a few, but acknowledged by everybody.

Marriage patterns already reflected the position achieved by such Italians. When, after his victory over Rosas, General Urquiza finally decided to marry, he chose for his wife the daughter of the Genoese merchant-supplier of his army, and this was not considered a *mésalliance*. Demarchi, a druggist from Lugano and sometime honorary consul of Sardinia, married the daughter of General Facundo Quiroga; again, nobody in his senses felt that he had married above himself.

As a consequence of the fall of Rosas, the position of this early Italian elite acquired an explicit political dimension. The rise of the liberal-national movement in the peninsula and of political liberalism in Argentina brought to the fore the ideological affinities between this elite—fiercely nationalistic, liberal, and anticlerical, and only superficially reconciled, if at all, with the monarchy of Savoy—and the new Argentine political leadership. This was again reflected in family alliances; Caprile became a relative by marriage of General Mitre, the first president of a unified Argentina, and this hospitable lineage—one of the oldest in Buenos Aires, but conspicuously impecunious—also integrated Dragos and Astengos in its fold.

Very soon the older immigrant group was overwhelmed by the sheer mass of

³ On the position of Sardinian subjects in Buenos Aires, see the very detailed report—"Rapporto sul commercio Sardo, colla Confederazione Argentina, e la Banda Oriental (Sud America) redatto da Carlo Belloc Vice Console de 1^a Categ^{ia} già attaccato al Consolato Generale di Buenos Aires," dated in Genoa, February 10, 1851—in the Archivio di Stato (Torino), Consolati Nazionali, Buenos Aires, mazzo 1.

⁴ Picolet d'Hermillon, Sardinian consul and chargé d'affaires, Buenos Aires, to the minister of foreign affairs, April 28, 1846, Archivio di Stato (Torino), Consolati Nazionali, Buenos Aires, mazzo 1.

newer arrivals; even then, and for decades to come, its elite managed to maintain unchallenged leadership of the vast Italian community in Argentina. Thanks to its economic superiority and the links it had built with the new Italy and the new Argentina, it kept close control of an associational and journalistic network that—as Grazia Dore has convincingly shown in her perceptive study⁵—imposed its own relentless nationalistic and anticlerical orientation on a community mainly made up of deeply Catholic and very superficially nationalized peasants.

By the 1880s, while in places like Córdoba the Italian consul still spoke in his official capacity to rallies of the locally dominant political faction, evoking commonly shared ideals, elsewhere signs of new strains were becoming evident (they are reflected, for instance, in the bittersweet comments offered by Sarmiento on the Italians in Buenos Aires in his polemical articles on immigration issues).⁶ In the following decade the political dimension of the Argentine-Italian relationship was progressively eroded by the conservative reorientation of the Argentine political elite; its demise was officially acknowledged in the new century, when October 12 was declared a national festivity, not as Columbus Day but as Día de la Raza, thus offering the first public homage to the Spanish (and implicitly Catholic) roots of Argentine nationhood. By then the liberal-national consensus had been effectively challenged among the Italians of Argentina by the new working-class ideologies. But the exhaustion of this half-century-old link is just one aspect of much vaster changes.

In 1852 Argentina had been in many ways a colonial polity and society, in which foreigners were typically better respected than natives (as General Mansilla later remarked, under his uncle Rosas it would have been helpful to have had a consul for Argentines; twenty years after Rosas's fall, foreign consuls were still busy protecting their nationals from administrative arbitrariness and police brutality, against which, of course, nobody bothered to shelter the Argentine victims); foreigners were also assumed to belong to the respectable classes: in José Mármol's novel *Amalia* (1851), while the rustic landowners are satirized or patronized, the arbiters of polite style are the widow and orphaned daughter of a French tradesman. It had also been an unruly country, ineffectively controlled from the sieges of government and trade.

Now all of this had changed: the post-Rosas ruling elite, after having imposed its harsh rule on the interior, was witnessing with dismay the emergence of the *clases dangereuses* in its own capital. It feared both the expanding middle class's upward mobility and the sullen restlessness of the popular classes. Behind both, it saw the newly immigrant masses, eager to inherit the earth. The changes within the socioeconomic elite were equally important: the landowners had finally got into money with a vengeance; some of them belonged among the immensely wealthy by world—and not just local—standards, and even the less affluent could afford a cosmopolitan life of leisure. Gone forever were the times when an apothecary from Lugano was considered a good matrimonial prospect for the daughter of a large landowner who was also a national political figure.

⁵ Dore, *La Democrazia italiana e l'emigrazione in America* (Brescia, 1964), 111–27, *passim*.

⁶ Domingo F. Sarmiento, *Condición del extranjero en América* (Buenos Aires, 1916).

By that time the Argentine economy was falling under the sway of foreign-based and -owned trade and transportation companies. For economic power they were more than a match for the import-export merchants who had settled in Buenos Aires immediately after its opening to world trade, in 1809, and who had been the leaders and pacesetters of the European community, in whose middle ranks not a few Italians had found a respectable place. But the transient local agents of such firms did not inherit the leading social position those merchants had enjoyed for so long. The landed elite was now alone at the top of Argentine society.

All this bode ill for the position of Italians in Argentina. It was not merely that wave after relentless wave of new immigrants confirmed the new image of the Italian as the poorest among the poor. The older Italian economic elite did not weather the transition as successfully as some of the Irish and Basque, who had acquired extensive land bases. By 1850 Italians had a stranglehold on river navigation, but Argentine expansion was shaped by the railways. By 1890 the very few Italians who somehow managed to reach the top were seen at best as exceptions and—more frequently—as irritating upstarts, the vanguard in a silent siege against the high places in Argentine society—a siege that might succeed, so the fears went, because of the sheer numbers of the plebeian invaders from overseas.

The puzzling figures of the 1914 census reflect—but not necessarily illuminate—this ambiguous historical progress, about which we really know much less than this necessarily schematic sketch suggests. Understanding it better would offer some basic keys to the Italian role in Argentina. That is, it would allow us to find out not only how well Italians really did but why in a country that Luigi Luzzatti could describe in 1900, in turgid style but with almost no hyperbole, as “blood from our blood and bones from our bones,”⁷ the trace of this massive Italian presence, while undoubtedly pervasive, is so elusive.

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⁷ Luzzatti, “Gli accordi commerciali dell’Italia con gli Stati delle Americhe,” in *Italia coloniale* (May 1900), as quoted in Dore, *La Democrazia italiana*, 200.

Reply:

I VERY MUCH APPRECIATE THE COMMENTS AND CRITICISMS provided by Jorge Balán, John D. Gould, and Tulio Halperin-Donghi, since their analyses give me an opportunity to clarify my position on several issues. Although they seem to accept my more basic findings of comparative differences and the role of American labor markets in determining patterns of economic integration and mobility, each author has suggested either alternative lines of argument or interesting areas for future research.

To begin with, I think all three scholars are concerned about my discussion of the relative differences apparent in the Italian regional origins and of the role played by American labor markets in determining these origins. They also have some question about the relative weight of American markets in explaining subsequent patterns of integration. The key factors in the first two questions revolve around the issue of literacy rates and the causes for the possible differences in the direction of the immigrant streams. While in most socioeconomic variables the regional differences are small, there are still significant differences between the *nord* and the *mezzogiorno* in literacy rates, which appear to be reflected in modest differences among migrants arriving in the Americas. But is this one factor sufficient to explain later patterns of integration? Given the dominance of unskilled laborers as migrants to all areas of the New World, it appears that relative rates of literacy had little immediate impact on agricultural laborers or unskilled urban workers. Moreover, the rates of illiteracy were still high enough in both immigrant streams so that just under half of the immigrants were illiterate in the Argentine case and just over half in the U.S. case. Finally, although literacy itself in the first generation may have favored the Argentine labor pool, this had little impact on second-generation Italo-Americans, as educational opportunities in both receiving societies appear to have been the same for the children of immigrants.

As I think all of us would agree, the regional statistics mask serious internal variations. Those who migrated often were the more advanced and dynamic populations in each region. Thus, the actual selection processes may have operated to homogenize even these differences, although such an assumption must await verification in far more detailed studies on local economic and social conditions in this period than are currently available in the Italian literature.

In terms of the destination preferences found in different regional migrations, I think that my commentators may have overstated my position on the influence of American market conditions. In all cases from Germany to Argentina, Italian

unskilled laborers were the primary migrants, and their migration could not and did not occur unless the labor markets in the receiving countries had a strong demand for such workers. But the timing and evolution of migration flows initially did not lead to a uniform participation across regions. Local Italian participation was greatly influenced by local conditions, just as the external market demand was determined by foreign considerations. The relative preference patterns that then developed as region A began to supply country X as both came into the market at the same time created a certain initial bias, even as countries Y and Z began opening their markets. These early preferences were then reinforced by what Professor Gould calls the “feedback” mechanism (page 336). Over time, however, as knowledge of the world market became more generalized throughout Italy and as all regions began to participate fully, regional preferences tended to break down and a more open market developed. This appears to be what was happening in Italy by the eve of World War I, and, as this occurred, the demands of foreign labor markets became an ever more important factor in influencing Italian migration. Thus, the initial opening up of the U.S. labor market to Italians did not immediately lead migrants from all regions to participate equally in supplying that market, although the movement over time was in that direction.

I also agree that intra-European migration is not to be ignored in this comparative case. But, as the data in my Table 3 (page 310) suggest, a key turning point occurred in the latter part of the 1880s, when the Americas became the destination for the majority of all Italian emigrants. Moreover, even as the primary movement of northerners continued to be to Switzerland, France, Austria, and Germany, the total number of northerners crossing the Atlantic did increase with every decade, despite their decline in relative importance. Also given the role Italian workers played in Europe, it can be argued that there was really relatively little structural difference between the intra-European and intercontinental streams. To Western Europe went mostly adult males, and primarily unskilled workers, just as to the Americas. While the minority who were skilled building workers may have been slightly more important among those going north than among those heading west, their rates of repatriation were even higher than those of their American-bound compatriots.

In questioning the major influences on the comparative differences in the rates of economic integration in America, the commentators have offered a number of interesting suggestions. I agree with Professors Halperin-Donghi and Balán that the early Italian community in Argentina was essentially commercial and elite. But it really differed little from the pre-1880 Italian community in the United States. This, too, in the early nineteenth century was primarily an urban, commercial business-class group, which was overwhelmingly northern Italian in origin, and Genoese at that. Although these communities may have established some of the norms for integration of later arriving migrants, in neither case can the activities of the early immigrants be seen as determining the patterns of post-1880 economic integration. In both nations as well, the rise of native elite prejudice against Italians after the beginning of mass immigration counterbalanced whatever more positive role the relative importance of the earlier merchant communities might have had.

Professor Halperin-Donghi's reminder about the constraints faced by Italo-Argentines in relation to those of other immigrants is well worth stressing. In comparing Italian achievements in Argentina with those in the United States, one can overlook their still difficult situation within Argentina itself compared to other immigrant groups. Thus, it is clear that, when the national statistics are broken down, it was the wealthier and more established zones that had the lower rates of Italian ownership and the newer and poorer ones that most likely had the heaviest representation of Italian immigrants. But, even with these differences and constraints more precisely laid out for Argentina, a similar breakdown for the United States would probably leave the comparative differences basically the same. Moreover, while Italians did not do as well as some of the smaller, competing immigrant groups, they did better than the Spaniards, who were the other mass immigrant group in Argentina.

Again, I can only agree with both Professors Halperin-Donghi and Balán that the available national Argentine statistics leave many crucial issues unexamined. For example, while the age and nationality of residents are available for property owners at the national level (see Table 9, page 322), the 1914 census does not provide the same breakdown for urban property owners. If this age adjustment could be done, then the relative importance of property owners among native-born Argentines would improve considerably, since they had a much higher proportion of children than did the predominantly adult immigrant Italians. Conversely, the relative importance of urban property owners among the latter would decline somewhat.

Professor Balán has also offered a number of competing factors that may have influenced comparative integration and mobility rates more than the nature of the local labor markets. He suggests, for example, a possible difference in stratification systems operating in the two American countries. But, as far as can be determined, these systems were more similar than different, and the differences are insufficient to explain the comparative Italian experience. Both were open, capitalist economies and societies where prestige and status were primarily determined by achieved versus ascribed factors.

He also asks whether the market selected the immigrants or the immigrants self-selected the markets and, if the latter, whether those with better original skills therefore did better in later integration. Without repeating all of my previous argumentation, I can simply state that the overwhelming majority of Italians were unskilled laborers and that neither American market initially signaled a need for any specialized skills, which could have resulted in selective migration. For engineers, doctors, and lawyers, of course, there is no question that careful attention was given to everything from local American markets to licensing regulations, and these professionals self-consciously sought the best conditions possible. But such immigrants formed a very small segment of both movements.

Just as I hold that a career- versus a wage-oriented immigrant did not exist prior to migration for the majority of Italians, I am also not so convinced that the return migration flows just reflected differences in the labor markets alone. The fact that repatriation was high in every American and European country to which Italians

went indicates that almost all Italian immigrants, whether singly or in family groups, did think about possible return to Italy, just as many thought of making it in the new society. Even in the unique case of Brazil, where government-sponsored migration supported a heavy family predominance, return migration eventually equaled that of the United States and Argentina.

Furthermore, there remains the difficult problem of separating out the temporary and seasonal labor migrants from the more committed and permanent workers. These seasonal migrants were to be found not only in the European nations but also in U.S. cities under the direction of *padroni* and in the Argentine wheat fields. As Massimo Livi Bacci has shown, the returnees had a much higher ratio of males than the entering migrants did and were overwhelmingly of recent arrival as compared to the resident Italian immigrants. Unfortunately, in this subject, as in so many others, Livi Bacci's work on the United States remains the only detailed analysis of the crucial demographic characteristics of any Italian immigrant population of this period. But how to distinguish the clearly temporary from the committed immigrants who repatriated and to determine the latter's motivations and decisions to return are areas that will have to be explored by other methods. This will, of necessity, involve a re-examination of the often conflicting Italian and American statistics as well as the development of data from other sources.

That more work of a quantitative and qualitative nature must be done is quite evident, I believe, from my general survey of the available national statistics and from the discussion of the three commentators. But, within the context of a more intensive study of the Italians in each national history, their relative role in other receiving societies should also be constantly kept in mind. By so doing, a much more comprehensive understanding can be achieved of both the nature of the individual immigrant experience and the role of the acculturation process in each national society. I also believe that the comparative study of labor markets and working conditions in each host country is essential to understanding the immigrant experience. It is this fundamental perspective that in the more recent historiography has not been sufficiently stressed.

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Research Note

Who Composed “Sazonov’s Thirteen Points”? A Re-Examination of Russia’s War Aims of 1914

WILLIAM A. RENZI

DURING THE FIRST WORLD WAR the Allies’ leadership articulated only two comprehensive sets of war aims—the Fourteen Points outlined by President Woodrow Wilson and the Thirteen Points attributed to Russian Foreign Minister Sergei D. Sazonov. In mid-September 1914 Sazonov is said to have enumerated a list of specific war aims, which Emperor Nicholas II in effect ratified in a slightly expanded and modified form during a November audience of the same year. Since this document became known in the 1920s, scholars have almost universally accepted Sazonov’s thirteen-point program as authentic and have cited and analyzed it in numerous works, ranging from studies of Russian war aims and foreign policy during the conflict to textbooks in Russian history.¹ But there is now some reason to doubt that Sazonov did indeed prepare this program, at least as it is known to date. Documents that have become available during the last ten to fifteen years at the French Foreign Ministry, the Institut de France in Paris, and the Public Record Office in London justify concluding that the long-accepted version of Sazonov’s program is misleading and inaccurate, that Nicholas II almost certainly neither confirmed nor ratified it, and that it never constituted official Russian policy.²

Research Note is a new feature in the *AHR*; the first article published under this rubric was John F. Bratzel and Leslie B. Rout, Jr.’s “Pearl Harbor, Microdots, and J. Edgar Hoover,” which appeared in the December 1982 issue (87: 1342–51). The editors invite the submission of short articles on recent archival discoveries or other revelations of major historical significance. On acceptance, an article will appear in the next available issue. Prospective authors should write the editors for a copy of the “*AHR* Guidelines for Authors.” THE EDITOR.

This article was made possible by grants from the American Philosophical Society and the American Council of Learned Societies. I would like to acknowledge with gratitude the assistance I received from Mrs. Hannam, executive director of the Photographic Section, Public Record Office, London (Kew), in providing me with a copy of George Buchanan’s letter of September 13, 1914, to Arthur Nicolson.

¹ See, for example, Alexander Dallin *et al.*, *Russian Diplomacy and Eastern Europe, 1914–1917* (New York, 1963), 80–82, 12–33; C. J. Smith, *The Russian Struggle for Power, 1914–1917* (New York, 1956), 46–48; Albert Pingaud, *Histoire diplomatique de la France pendant la grande guerre*, 1 (Paris, 1928): 126–29; Leo Valiani, *La Dissoluzione dell’Austria-Ungheria* (Milan, 1966), 178; and Michael Florinsky, *Russia: A History and an Interpretation*, 2 (New York, 1955): 1347.

² Specifically, these include the files of the French Foreign Ministry per se as well as the Maurice Paléologue and Théophile Delcassé MSS at the ministry, the Stephen Pichon MSS in the library of the Institut de France,

THE NATURE OF THE EVIDENCE establishing the authenticity of the Thirteen Points and their ratification by the tsar is questionable. All but one of the documents supporting the traditional narrative flow from the pen of one man—Maurice Paléologue, France's ambassador to St. Petersburg. They consist of passages in his memoirs, published in 1921, and of telegrams exchanged between the French embassy in the Russian capital and the French Foreign Ministry. Unknown to the French, these telegrams were intercepted and decrypted by a Russian intelligence agency and were published by the Soviets after the Revolution of 1917.³

According to Paléologue's memoirs, in a reference frequently neglected by historians, Sazonov's first mention of war aims occurred during a luncheon conversation the two men had at the French embassy on August 20, 1914. During the course of their dialogue, Sazonov allegedly stated that Russia was fighting to achieve nothing less than the complete destruction of German militarism—a goal the foreign minister proposed to accomplish, at least in part, by imposing peace terms that included restoring Alsace-Lorraine to France, enlarging Belgium, "reconstructing" the province of Hanover, returning Schleswig to Denmark, creating an independent Bohemia, and partitioning Germany's colonial empire among France, Britain, and Belgium.⁴

No trace of a telegram, dispatch, or private letter from Paléologue to any member of the French government, reporting his conversation with Sazonov, can be found in the archives of the Quai d'Orsay and the Institut de France or in any of the several relevant Soviet documentary collections. Nor did Paléologue refer to this conversation, which would have been the foreign minister's first comments about war aims, in any of his subsequent wires dealing with and summarizing Sazonov's statements on the subject. At the least, the conversation was notional; at the most, Paléologue himself advanced the war aims he later attributed to Sazonov. All that this August reference to war aims can demonstrate is that the ambassador, whose three-volume memoirs are obviously paraphrased diary entries, speculated about the terms of peace when the war was only a few days old.

Not until September—after another conversation with Sazonov—did Paléologue inform the French government of the thirteen-point program. The ambassador telegraphed the French foreign minister, Théophile Delcassé, on Monday, September 14, that Sazonov had (evidently that same day) revealed to both the French and British ambassadors a program of war aims that Paléologue described as tentative and therefore, in effect, subject to negotiation. The text of the program can be found among Russian decrypts of Paléologue's telegrams to the Quai d'Orsay, and it merits reproduction in full:

1. The three allied powers have as their primary goal the destruction of German power and the German desire for military domination;

and the British Foreign Office files, including the Edward Grey MSS, the Harold Nicolson MSS, the Francis Bertie MSS, and cabinet documents, in the Public Record Office. I have also used a few telegrams and dispatches deposited in the Kriegsarchiv, Vienna, and in the Archivio Storico Diplomatico and the Archivio Centrale dello Stato, Rome.

³ See David Kahn, *The Codebreakers: The Story of Secret Writing* (New York, 1967), 621.

⁴ Paléologue, *La Russie des tsars pendant la grande guerre*, 1 (Paris, 1921–22): 92–93.

2. Territorial modifications must be determined by the principle of nationality;
3. Russia will annex the lower course of the Niemen River and the eastern portion of Galicia; it will also annex to the kingdom of Poland eastern Posen, southern Silesia, and the western portion of Galicia;
4. France will regain Alsace-Lorraine, and, if it so desires, a portion of the Prussian Rhineland and of the Palatinate;
5. Belgium will receive a significant territorial increase in the vicinity of Aix-la-Chapelle;
6. Denmark will regain Schleswig-Holstein;
7. The kingdom of Hanover will be restored;
8. Austria will be divided into a tripartite monarchy, comprised of the empire of Austria, the kingdom of Bohemia, and the kingdom of Hungary; the Austrian empire will include only its hereditary provinces, the kingdom of Bohemia will be comprised of present-day Bohemia as well as Slovakia, and the Hungarian kingdom will have to reach an understanding with Romania concerning Transylvania;
9. Serbia will annex Bosnia, Herzegovina, Dalmatia, and the northern portion of Albania;
10. Bulgaria will receive from Serbia territorial compensation in Macedonia;
11. Greece will annex the southern portion of Albania, with the exception of Valona, which will fall to Italy;
12. Britain, France, and Japan will divide the German colonies; and
13. Germany and Austria will pay war reparations [*une contribution de guerre*].⁵

The account of the same meeting by the British ambassador, George Buchanan, differs significantly from the thirteen-point program reported by Paléologue. Not only are the "points" not identical, but the tone is also quite different: Buchanan imparts a more tentative and informal cast to Sazonov's words. And Buchanan dated the conversation Saturday, September 12—two days earlier than Paléologue's telegram to the Foreign Ministry in Paris implies. Moreover, he did not consider the conversation important enough to warrant a formal or a speedy report to London. Instead, he casually mentioned it among many other topics in a personal letter of September 13 to Arthur Nicolson, then second in command at the British Foreign Office. Buchanan opened the letter by stating that he was writing largely because he had found reliable couriers. His account, which probably took weeks to reach London via the dispatch bag, reads,

Though it is too early to speak of the changes which must be made in the map of Europe after the war it may interest you to know Sazonov's views on the subject. In a purely academic conversation which Paléologue and I had with him yesterday, he remarked that he was personally opposed to the idea, which finds favor in many quarters here, of claiming East Prussia up to the Vistula for Russia. Such an accession of territory with its large German population would prove a source of weakness and not of strength. On the other hand he would like Russia to obtain the mouths of the Nieman. The greater part of Posen with the Polish districts of Galicia and Silesia would, he hoped, be united with Poland, while Russian Galicia would be incorporated in Russia proper. The basis of final settlement should be that

⁵ I have been unable to locate the original of Paléologue's telegram in the archives of the Quai d'Orsay. The file that contained it was removed from Paris during the Nazi occupation of the city, and some documents were missing from the file when it was returned after the Second World War. I have reconstructed the telegram from the following sources: *Mezhdunarodnye otnosheniia v epokhu imperializma: Dokumenty iz arkhivov tsarskogo i vremennogo pravitel'stva, 1878–1917 gg.*, 3d ser., 6 (Moscow, 193x), docs. 247–54, 256–57; *Un Livre noir: Diplomatie d'avant-guerre et de guerre d'après des archives russes, 1910–1917*, 3 (Paris, 1926): 11–13; and Pingaud, *Histoire diplomatique de la France pendant la grande guerre*, 126–28. Pingaud's diplomatic history is the most important, because Pingaud had access to the French archives in the 1920s. For the fate of the Foreign Ministry's archives during World War II, see Pierre Renouvin, "Les Buts de guerre du gouvernement français, 1914–1918," *Revue Historique*, 235 (1966): 1–2.

of nationality. France would recover her lost provinces; Serbia would acquire Bosnia and the Herzegovine with part of Dalmatia giving her access to the Sea; Belgium should be given some compensation for her heroic sacrifices and Denmark might perhaps recover Schleswig and Holstein. Great Britain would, he imagined, take most of the German colonies. Austria would be converted from a Dual Monarchy into a Triple Monarchy and the Crowns of Hungary and Bohemia would be vested in the Emperor under a personal Union. Whether Austria retained Transylvania and the Trentino would depend on the action of Roumania and Italy. Sazonov alluded vaguely to the necessity of regulating the question of the Straits in a manner satisfactory to Russia and Roumania and also suggested that England, France, Russia and Belgium should conclude a defensive Alliance for the maintenance of European Peace.⁶

Paléologue's memoirs also record that Nicholas II confirmed and elaborated on the ambassador's version of the program a little over two months later during an audience granted expressly for the purpose. Paléologue described the audience both in six consecutive telegrams to his superiors in France and later in his memoirs. According to the ambassador, Sazonov informed him on the morning of November 21, "The emperor will receive you presently, at 4:00 p.m. Officially, he has no declaration to make to you. But he wishes to speak with complete candor and liberty, and I should advise you that the audience will be a long one." Nicholas received the ambassador in his private study at Tsarskoe Selo. According to Paléologue, the tsar began a discussion of the eventual peace, indicating his desire for "dictated" terms for the Entente's enemies. Its ultimate objective would be the destruction of German militarism, "the termination of a nightmare that Germany has forced us to live with for more than forty years." Nicholas stated that he would approve in advance any peace terms the French and British governments might pursue.⁷

Paléologue thanked Nicholas for this declaration, adding that the French government would certainly be sympathetic to any specific Russian desiderata. According to the ambassador, the emperor responded, "That encourages me to confide in you all of my thoughts." The tsar spread out a map of Europe on the table and began to speak in a less formal tone. His general staff, he revealed, wished to annex Prussia as far west as the mouths of the Vistula River, but he thought this excessive and would decide the matter later. Pomerania and possibly "a fraction" of Silesia, however, should be incorporated into a newly created Polish state. Galicia and southern Bukovina should be annexed to Russia.⁸

Turkey had entered the conflict only at the beginning of November, but, again according to Paléologue, Nicholas appeared to have given the Turkish question considerable thought. Armenia in particular posed a problem for the Russian empire. The tsar would annex Armenia if its population expressly requested Russian sovereignty; otherwise, Russia would "organize an autonomous regime" for the Armenian people. The Turks would be completely expelled from Europe, while Russia would be guaranteed "free passage of the straits." Constantinople would become a neutral city subject to international control, while the Turkish government would be obliged to seek a new political capital, either at Angora or at

⁶ Buchanan to Nicholson, September 13, 1914, private letter, Public Record Office, London, Foreign Office [hereafter, FO] 800, 375.

⁷ Paléologue, *La Russie des tsars*, 195–98.

⁸ *Ibid.*, 198–99.

Konia. All tombs and mosques on the Bosphorus, however, would be respected. Southern Thrace, as far as the Enos-Media line, would fall to Bulgaria; the remainder of Thrace, including its littoral but excluding Constantinople, would become Russian territory. Paléologue inquired about France's "patrimony" in Syria and Palestine, suggesting that the French government might want to safeguard it; Nicholas readily agreed.⁹

The tsar then turned to a map of the Balkan peninsula. Still according to Paléologue, Serbia was to receive Bosnia, Herzegovina, Dalmatia, and the northern portion of Albania. If Bulgaria remained neutral (*si elle reste sage*), it would receive territorial compensation in Macedonia from Serbia. Nicholas then put the map aside and asked the ambassador for his views on the fate of the Austro-Hungarian empire. Paléologue opined that, if the Russian armies scaled the Carpathians, Italy and Romania would enter the conflict against their former ally.¹⁰ The two men agreed that this would be the end of the Dual Monarchy. Once deprived of Transylvania, the emperor thought, Hungary would have difficulty in ruling the Croats. Bohemia would "at the least" reclaim its political autonomy, reducing the Austrian portion of the empire to its "hereditary states," the Tyrol and the region of Salzburg.¹¹

At this point the tsar fell silent, lost in thought. Then, after glancing at a portrait of his father, he declared, "In Germany, above all, great changes will have to be executed. As I have told you, Russia will annex former Polish territories and a portion of eastern Prussia." France in turn would receive Alsace-Lorraine and perhaps the Rhineland. Belgium would acquire "an important augmentation" of territory in the Aix-la-Chapelle. Schleswig, including the Kiel canal, would be given to Denmark. Hanover would revert to the British crown, while France and Britain could divide Germany's overseas possessions "as they wished." What remained of Germany would be free to organize and govern itself as it desired, except that the Hohenzollern dynasty no longer merited "imperial dignity" and Prussia had to become "a simple kingdom." Finally, the tsar remarked that he attached "the greatest importance" to maintenance of the alliance between Petrograd, Paris, and London as the only means of assuring lasting peace after the war. Then, following a discussion of William II's personality, Nicholas terminated the audience, after a conference that had lasted over two hours.¹²

BOTH INTERNAL AND EXTERNAL EVIDENCE INDICATE that, although Sazonov discussed war aims in general with the British and French ambassadors in mid-September 1914, he did not articulate the precise, detailed program that Paléologue attributed to him. And the monarch's subsequent conversation with Paléologue was clearly far from what the ambassador claimed.

⁹ *Ibid.*, 199–200.

¹⁰ Rome and Bucharest had, in fact, reached a written agreement on September 23, 1914, pledging each state not to abandon neutrality without giving the other ten days warning. See Glenn E. Torrey, "The Rumanian-Italian Agreement of 23 September 1914," *Slavonic and East European Review*, 64 (1966): 403–20. As Torrey pointed out, the accord soon became an open secret.

¹¹ Paléologue, *La Russie des tsars*, 195.

¹² *Ibid.*, 200–05, and Paléologue to Delcassé, telegrams 957–62, November 22, 1914, Quai d'Orsay, Paris [hereafter, Qd'O], Delcassé MSS, 1, ff. 146–47.

Understanding how such a distortion of the historical record was possible begins with Paléologue's personality. In 1911, when Paléologue was the French ambassador in Sofia, the Austrian military attaché there described him to General Franz Conrad, chief of the Austrian general staff:

About fifty years old, unmarried, descended of a Jewish family (Baliol), which migrated from Jassy to France. [He is] prominent, vivacious, well educated, but displays a fantastic imagination and is an author of novels. [He] permits his novelist's imagination to run away with him when he interprets insignificant military or political events, and, for those who do not know him well, he is therefore dangerous as a source of information. In general, he is uninterested in Austria-Hungary but displays professional interest in Franco-Russian relations.¹³

In February 1914, shortly before Paléologue presented his credentials to the Russian court, Ambassador Buchanan limned a strikingly similar sketch of the French ambassador:

M. Paléologue, who has only just arrived, was my colleague at Sophia during the Balkan crisis of 1908–9. He is a very cultivated man, a writer of light romances, as well as of books of a more serious vein; but, unless he has changed since I knew him, his vivid imagination is apt to run away with him and disposes him to take a fanciful and exaggerated view of the political questions with which he has to deal. He was transferred from Sophia to the Quai d'Orsay [in 1912] by M. [Raymond] Poincaré, who is an old schoolfellow of his, and he has recently acted as political director of the Ministry for Foreign Affairs. It was only after M. [Pierre de] Margery had declined the post that M. Paléologue was named Ambassador at St. Petersburg.¹⁴

Baptized Georges Maurice Paléologue in 1859 and descended of former Byzantine aristocracy, he entered the French foreign service in 1880. Paléologue soon came to enjoy a second career and minor reputation as a writer, beginning in the late nineteenth century with fiction of a highly imaginative nature and later turning to biographical works on figures from Camillo Cavour to the empress Alexandra. These more "serious" titles are really no more than popularized, historical romances.¹⁵ His extant telegrams, dispatches, and memoranda—written from 1908 to 1920 and now in the archives of the Quai d'Orsay—disclose a personality prone to dramatic factual reports and to exaggerated accounts of his personal influence and control over events. But Paléologue's diplomatic career did not depend on talent. His posting to the Quai d'Orsay, eventually as political director, and his subsequent ambassadorship in the Russian capital undoubtedly derived from his longstanding association with Raymond Poincaré, a friend since high school, who was elected president of the French republic in January 1911.¹⁶

¹³ Zhranilović to Conrad, May 26, 1911, dispatch 58, Kriegsarchiv, Vienna, Evidenz Büro, Fasz. 5585.

¹⁴ Buchanan to Grey, February 12, 1914, dispatch 43, FO 371, 2091.

¹⁵ My evaluation is based on a reading of the following Paléologue titles: *Sur les ruines* (Paris, 1897); *La Cravache* (Paris, 1904); *Le Point d'honneur* (Paris, 1907); *Le Cilice* (Paris, 1921); *La Roman tragique de l'empereur Alexandre II* (Paris, 1923); *Cavour, un grand réaliste* (Paris, 1926); *The Tragic Empress: A Record of Intimate Talks with the Empress Eugenie, 1901–1919* (New York, 1928); *Alexandra-Feodorovna, impératrice de Russie* (Paris, 1932); and *Elisabeth, impératrice d'Autriche* (Paris, 1939).

¹⁶ See Poincaré to Paléologue, March 9, 1915, private letter, Bibliothèque de l'Institut de France, Paris [hereafter, BIF], Pichon MSS.

According to Buchanan, Paléologue's appointment to Russia "was not very agreeable to M. Sazonov."¹⁷ And the Russian's reasons are not difficult to posit. Paléologue may have enjoyed influence and may have had high-placed friends and family in France, but, compared to his immediate predecessors, he lacked the experience expected of the head of France's most important embassy. In contrast, Sazonov's position as foreign minister had been seriously undermined in 1911 by the assassination of his brother-in-law, Premier Peter Stolypin, and evidently by the dislike of the empress as well.¹⁸ Periodic rumors had it that he would soon be forced to resign. Paléologue was a talkative extrovert, while Sazonov was a relatively taciturn man plagued by poor health.¹⁹ That Paléologue occasionally broached topics that Sazonov would have preferred to avoid is, therefore, not surprising. In fact, very shortly after the war began, Gaston Doumergue, then the French foreign minister, found it necessary to caution Paléologue concerning his daily conversations with Sazonov. In mid-August Doumergue specifically admonished Paléologue to keep his imagination in check and not lead Sazonov toward unintended statements or conclusions concerning war aims: "I cannot at this moment stress enough that you maintain prudence and moderation in your conversations with Sazonov. . . . Your role must be essentially a moderating one. Your duty [*devoir*] is to avoid any element in your conversations that could appear provocative or encourage premature initiatives or inadvisable projects."²⁰

Once the war began, Paléologue and Buchanan saw Sazonov twice daily, usually together.²¹ As his memoirs clearly reveal, Buchanan, like Sazonov, was unhappy with the French ambassador's appointment. The Briton was a practitioner of traditional nineteenth-century diplomacy; hence, he frequently found himself at a considerable disadvantage in their conversations *à trois*. Early in 1915, for example, Buchanan excused his failure to execute a particular *démarche* precisely as planned with the explanation, "I have so few opportunities of seeing Minister for Foreign Affairs alone that I preferred to write [to him], and I find it very difficult to make any impression on him when my French colleague is present."²²

Paléologue's own evidence contains the same erratic quality that was manifested in his personality. The French ambassador made no mention of the thirteen-point program in his highly detailed memoirs (which were published in 1921, just prior to the Soviet publication of his September 14 telegram); only his wire contains the Thirteen Points.²³ His account of his audience with the tsar on November 21,

¹⁷ Buchanan to Grey, February 12, 1914, dispatch 43, FO 371, 2091.

¹⁸ *Letters of the Tsaritsa to the Tsar, 1914–1915* (London, 1923), 143, 205; and Giulio Melegaro to Antonio Di San Giuliano, September 19, 1911, telegram 4518/117, Archivio Centrale dello Stato, Rome, carte Giolitti, busta 25, fasc. 64.

¹⁹ Melegaro to Di San Giuliano, September 19, 1911; Buchanan to Grey, February 15, 1914, telegram 53, FO 371, 2091; and Georges Louis, *Les Carnets*, 1 (Paris, 1926): 188.

²⁰ Doumergue to Paléologue, August 16, 1914, telegram 648, Qd'O, dossier Italie 555, f. 111.

²¹ Paléologue to Doumergue, August 17, 1914, telegram 449, *ibid.* Also see George Buchanan, *My Mission to Russia and Other Diplomatic Memories*, 2 vols. (Boston, 1923), 2: 63.

²² Buchanan to Grey, June 4, 1915, FO 371, 2245. Part of Buchanan's difficulties in dealing with Paléologue may have stemmed from the chronic ill health the Briton suffered throughout the period of his ambassadorship in Russia; *My Mission to Russia*, 1: 140–42.

²³ *Un Livre noir*, published sometime between 1922 and 1927, first made the thirteen-point program public property; see *ibid.*, 11–13.

however, appears in extended form both in his telegrams to his superiors and in his memoirs. But Buchanan's reports provide additional information to unravel the sequence of events. On November 15, the ambassador wired Edward Grey, secretary of state for foreign affairs, a description of Paléologue's recent professional activities in the Russian capital, and Buchanan's telegram indirectly sheds light on the authorship of the thirteen-point program:

My French Colleague has more than once in our conversations with the Minister for Foreign Affairs suggested that the three Powers should commence informal discussions of eventual conditions of peace and he yesterday spoke of submitting to the Emperor, with whom he is to have an audience this week, general line which these conditions should follow.

I always contended that we must concentrate all our energies on beating the enemy and that until our armies are much nearer Berlin than they are at present it is useless to discuss terms which we intend to impose on Germany.

I spoke to Minister for Foreign Affairs in the above sense today and expressed hope that he would discourage French Ambassador's idea. His Excellency said that he quite shared my view and that he would warn the Emperor in case the Ambassador should broach the subject to His Majesty.²⁴

It is also evident that Paléologue's subsequent audience of November 21 took place at his own request. His excuse was the need to speak with the emperor concerning the recent political activities of Sergei Witte. A former premier and minister of finance, Witte had re-entered politics in 1913, advocating Germany's adhesion to the Franco-Russian alliance.²⁵ Once the war began, Witte personally undertook to convince Russia to terminate the conflict with Germany at once, since he believed the war could only end in Russia's defeat.²⁶

Unfortunately, the only reliable account of that November 21 audience, supposedly called to discuss Witte, is a sentence written by Buchanan—but even that comes second-hand (although it is doubtless accurate). Buchanan learned from Sazonov what had happened at Tsarskoe Selo, and he informed Grey, "In audience accorded to him on 21st November French Ambassador did not say a word about Witte, but spoke exclusively on the subject of eventual terms of peace."²⁷ Nicholas II was notoriously easy to influence. But Paléologue may also have put the Thirteen Points into the emperor's mouth in order to impress Delcassé, who evidently believed—rightly or wrongly—that all important matters of foreign policy were decided by the emperor.²⁸

The reaction in France to Paléologue's telegrams recounting the thirteen-point

²⁴ Buchanan to Grey, November 15, 1914, private telegram, FO 371, 2174. The remaining paragraph of Buchanan's telegram reads, "As French Ambassador is sure to revert to the subject again soon I should like to be in a position to tell him [Sazonov] the view which you take of the matter." Grey responded, "I agree with your view as expressed in the second paragraph [of your telegram]. Discussion of terms of peace is academic till war has progressed sufficiently to make Germany contemplate the most obvious terms of peace such as restoration of two lost provinces to France and evacuation of and reparation to Belgium. When that period arrives discussion of how far the Allies should go in the terms that they demand will cease to become academic and there will be many questions to be considered including the Turkish Question." Grey to Buchanan, November 16, 1914, private telegram, FO 371, 2174.

²⁵ "Note sur le comte Witte—Voyage de Monsieur Poincaré" (unsigned memorandum), July 12, 1914, Qd'O, dossier Russie 42, ff. 270–72.

²⁶ Guglielmo Imperiali to Sidney Sonnino, April 15, 1915, dispatch 2724/265, Archivio Storico Diplomatico, Rome, pacco 235.

²⁷ Buchanan to Grey, November 23, 1914, telegram 691, FO 371, 2174.

²⁸ Francis Bertie, memorandum of October 20, 1913, FO 800, 177.

program and his subsequent conversation with Nicholas is interesting. Delcassé appears to have realized at once that, at the least, Paléologue had not accurately transmitted the details of the program, and the minister may have suspected that his ambassador had initiated and probably dominated the conversation with Sazonov. At any rate, he did not answer the telegram that contained the Thirteen Points.²⁹ Undaunted, Paléologue telegraphed twelve days later that the Russian minister for agriculture, A. V. Krivoshein, had spoken to him of Russian war aims and had reiterated Sazonov's program.³⁰ When this telegram too failed to elicit a response, Paléologue went a step further and wired Delcassé in early October that Sazonov's deputy at the Russian foreign ministry, Maurice Schilling, had stated that Sazonov was "astonished" because Delcassé had not reacted to the program.³¹

This last message moved Delcassé to respond. With evident sarcasm, Delcassé took the relatively unusual step of composing in his own hand a telegram to Paléologue, replying that he received the program "with equal interest and sympathy" but abruptly concluding, "If the hour arrives for a more serious examination [of war aims], I will be ready to discuss the matter."³² Delcassé would scarcely have responded so tardily and in such language if he had believed Sazonov to be the sole—or even the principal—author of the program. Otherwise, his response would have constituted a serious diplomatic insult to a nation that was then France's most important military ally. Paléologue evidently realized that his bluff had been called, for on October 11 he telegraphed a single sentence to Delcassé reporting that Sazonov had found the reply satisfactory.³³ Since Sazonov knew the text of Paléologue's September 14 telegram via Russian cryptography, would he—if he had been the sole author or if it had represented actual Russian foreign policy—have been so acquiescent in allowing the matter to be dropped peremptorily?

Turkey's entry into the conflict almost certainly accounted for Paléologue's decision to revive the matter. On November 15, he related to Delcassé a conversation with Russian Premier I. L. Goremykin, who, the ambassador claimed, had just espoused a program similar to that of Sazonov. The premier had added, however, that Constantinople would have to become a free city under international control, that Russia intended to annex a portion of Armenia, and that the Triple Entente would have to be consecrated into a lasting alliance after termination of hostilities.³⁴ This time Delcassé acted promptly. Again he personally composed a wire to Paléologue stating that, although his own views were generally in concert with those of the Russian premier, it was premature to discuss peace terms.³⁵

A week later Delcassé received Paléologue's telegrams describing his conversation

²⁹ C. J. Smith asserted that both the French and British governments delivered a reply of a general nature to the thirteen-point program via the Russian ambassadors in France and Britain and that the French cabinet had discussed France's reply before delivery; *The Russian Struggle for Power*, 52–54. A careful translation and reading of the documents on which these conclusions are based, however, reveals them to be without foundation; see *Mezhdunarodnye otnosheniia v epokhu imperiazma*, docs. 267, 260–62, 284–85, 278–79, 287, 281–82, 372, 381–82.

³⁰ *Un Livre noir*, 17–18.

³¹ Paléologue to Delcassé, October 10, 1914, telegram 724, Qd'O, Série paix 58, f. 15.

³² Delcassé to Paléologue, October 10, 1914, telegram 214, *ibid.*, f. 15.

³³ Paléologue to Delcassé, October 11, 1914, telegram 731, *ibid.*, f. 16.

³⁴ Paléologue to Delcassé, November 15, 1914, telegram 919, *ibid.*, f. 22.

³⁵ Delcassé to Paléologue, November 15, 1914, telegram 381, *ibid.*, f. 23.

with the emperor. After a brief delay, he transmitted a complete account of the audience to Grey, almost certainly with a request that the British government take the lead in settling the matter.³⁶ In a message to Buchanan, approved before dispatch by Prime Minister H. H. Asquith, Grey stated that his government believed that the notes exchanged by the Allies on September 5, 1914, binding each not to conclude a separate peace, sufficed as a war program; further discussion of the matter was premature. Grey did state that Japan would have to be consulted on Far Eastern matters when the time came. He concluded, "Personally I attach great importance to maintaining as a guarantee of peace in Europe after the war is over the alliance between the French, Russian and British Governments that the aggression of Germany has brought into existence."³⁷ On November 25, Buchanan promptly telegraphed Sazonov's reaction: "Minister for Foreign Affairs agrees that notes exchanged on September 5th are sufficient for the moment and also that Japan will have to be consulted as regards Far Eastern settlement. Terms of peace dealing with European questions should, however, in his opinion be drawn up by England, France, and Russia alone."³⁸

Paléologue was notified of Buchanan's instructions and was probably present when they were executed.³⁹ Apparently, he failed to grasp that Sazonov, in accepting Grey's statements, had abandoned the mid-September program and had invalidated any commitments the emperor may have made during their audience. Or perhaps he resented Delcassé for penetrating his ruse. In any event, during the spring of 1915 he denounced the foreign minister directly to Poincaré in his first communication to the president since he had become the French ambassador to Russia, except for a brief telegram dispatched at the opening of hostilities that merely expressed faith in Poincaré's leadership.⁴⁰ Paléologue complained bitterly to his friend of the lack of response from Delcassé concerning the ambassador's audience of November 21, and he recapitulated at length his version of the conversation, claiming that it had taken place entirely on the tsar's initiative. He lamented that the Quai d'Orsay had elected to remain silent on the matter, concluding, "Thus when Sazonov asked me two or three times what impression the imperial confidences had produced on the French government, I could only respond, 'I have no idea.'"⁴¹

HERE THE TALE ENDS, at least as far as the documentation is concerned. Apparently, Paléologue never again mentioned in his telegrams to the Quai d'Orsay Sazonov's Thirteen Points or his discussion of war aims with the tsar, and the memoirs of Sazonov, Buchanan, Poincaré, Grey, and Asquith are completely silent on the

³⁶ Nicolson to Grey, December 4, 1914, private note, FO 371, 2174; Grey to Nicolson, undated private note, *ibid.*; and Nicolson to Grey, undated private note, *ibid.*

³⁷ Grey to Buchanan, November 25, 1914, telegram 1098, FO 371, 2174.

³⁸ Buchanan to Grey, November 30, 1914, telegram 720, *ibid.*

³⁹ Paléologue to Delcassé, November 27, 1914, telegram 988, Qd'O, Série paix 58, f. 34.

⁴⁰ Paléologue to Delcassé (for transmission to Poincaré), September 13, 1914, telegram 602, Qd'O, Guerre, dossier général 22, f. 171.

⁴¹ Paléologue to Poincaré, April 15, 1915, private letter, BIF, Pichon MSS, 3.

matter.⁴² Thus, although Paléologue may have drawn Sazonov into a general discussion of war aims, the Russian foreign minister probably did not authorize the French ambassador to transmit a specific, thirteen-point program to his superiors but, instead, spoke only in a general or hypothetical sense. Sazonov does not seem to have intended Paléologue to initiate formal negotiations with Paris; otherwise, a similar program would have been given to Buchanan to transmit to his superiors in London. If the emperor did make statements concerning Russia's desiderata, they were rendered invalid by Sazonov's subsequent statement to Buchanan, agreeing that a formulation of war goals was premature in the late fall of 1914. Sazonov quite likely would not have made that declaration to the British ambassador without having first consulted the tsar.

Paléologue's messages and statements concerning Russian war aims in 1914 are, nevertheless, not completely devoid of value, for the program does represent to an extent what was *unofficially* being bruited about in Petrograd. The gregarious Paléologue had established a number of personal contacts among the Russian aristocratic, ruling elite. And doubtless, after Turkey entered the conflict, Russia was moving fast toward requesting what Grey later termed "the greatest prize of the whole war," postwar possession of the Golden Horn.⁴³

Paléologue's inventive folly has deceived three generations of historians. Those who have maintained that Russia helped precipitate the conflict to realize overly ambitious, even grandiose, territorial gains have sometimes used Sazonov's Thirteen Points as the diapason of their proof. The incident suggests, furthermore, that none of Paléologue's writings can be considered reliable. Some scholars have already asserted that at the very end of July 1914 Paléologue deliberately deceived his own government concerning the extent of Russian military preparations. It is certain, at the very least, that out of miscalculation, perhaps born of panic, or by deliberate design Paléologue dispatched the telegram announcing Russian general mobilization to Paris via by far the slower of the two telegraph agencies at his disposal, and that his own explanation for this decision is simply untrue.⁴⁴ But now all of Paléologue's statements, observations, and judgments are suspect. Until a critical examination of his memoirs and at least the more important of his telegrams and dispatches has been undertaken, his work must be employed with the greatest care possible.

⁴² For these memoirs, see Sazonov, *Les Années fatales* (Paris, 1927); Buchanan, *My Mission to Russia*, volume 1; Poincaré, *Au service de la France: Neuf années de souvenirs*, 5 (Paris, 1926–33); Grey, *Twenty-Five Years, 1892–1916*, 2 (New York, 1925); and Asquith, *Memories and Reflections, 1852–1927*, 1 (Boston, 1928).

⁴³ Grey to Buchanan, March 11, 1915, telegram 43, FO 371, 2449.

⁴⁴ Especially see Luigi Albertini, *The Origins of the War of 1914*, 2 (London, 1953): 619–25.

Reviews of Books

GENERAL

AGNES HELLER. *A Theory of History*. Boston: Routledge and Kegan Paul. 1982. Pp. viii, 333. \$35.00.

Agnes Heller was a student of George Lukács in Budapest, but she is no simple follower of this or any other master. With a commendable directness she lists (pp. vii–viii) the writers to whom she feels a significant intellectual indebtedness, and they span the Atlantic as well as much of the ideological spectrum. But, she writes, “One author and one book stand out with their considerable impact on this undertaking: Collingwood and his work, *The Idea of History*. Before I read this book there was chaos in my thoughts regarding my subject matter: having finished it, I knew perfectly well what I was going to argue for. My deepest gratitude is due to this unjustly neglected author. This book is dedicated to his memory” (p. viii).

One is immediately reminded of Louis O. Mink’s remark, in what is now clearly established as the foundation for a proper understanding of Collingwood, that “So many people have called Collingwood an ‘unduly neglected’ thinker that he is coming to be surely the best known neglected thinker of our time” (*Mind, History, and Dialectic: The Philosophy of R. G. Collingwood* [1969], p. 1). And yet there is some justice to Heller’s observation, for the richness of Collingwood’s thought has by no means been fully explored, let alone exploited, and one can only applaud the undertaking of a scholar steeped in the Middle European intellectual tradition to make use of a philosopher whose most enthusiastic readers are chiefly to be found in the United States and Canada.

Such a reader will not be comfortable with this book, or even feel assured as to its direction and message, for it too readily bogs down in the elaboration of the author’s own special terminology, and the subsequent complex manipulation thereof. But the general burden is clear enough: theory of history must be distinguished from philosophy of history (Heller’s conception of which might be summarized as moral judgment and intention for the

future according to the supreme values enshrined in the philosophy) and from historiography (by which she means the writing of histories through correct explanation and interpretation of evidence). To her, theory of history must be oriented toward the future, with reform of society as its goal. Hence, she argues, it must be socialist and work toward socialism, but as a project rather than as a blueprint-ed objective.

All of this is far indeed from what we who are interested in the field ordinarily think of as theory of history, and far indeed from R. G. Collingwood. Yet there is a relationship. Collingwood was one of the few modern philosophers to examine and employ the concept of progress (*Idea of History*, p. 321 and following) as a historical process of mind (hence conceivably reversible), not as an inevitable natural evolution. Heller quotes Collingwood’s remark that where “there is gain without any corresponding loss, then there is progress. And there can be progress on no other terms” (*Idea of History*, p. 329). Heller incorporates this view in her program for “a socialist theory of history,” where the idea of socialism qua utopia is to serve to light the path to a better future.

In a word, we are presented here with a very complex form of attenuated Marxism, with the author’s preferences within the fold going to Bernstein in the past and Habermas today. Heller’s admiration for Collingwood is easily understood, for Collingwood effectively transcends the voluntarist-determinist antinomy that always shadows Marxism and he rests progress and the very hope for it in human thought. But it is not possible to understand Collingwood, and almost impossible not to distort him, by reading out of his large body of work only *The Idea of History*, as she apparently has. One may hope she will go on to the rest.

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HARRY J. AUSMUS. *The Polite Escape: On the Myth of Secularization*. Athens: Ohio University Press. 1982. Pp. xii, 189. \$20.95.

JEAN-PIERRE SIRONNEAU. *Sécularisation et religions politiques*. Summary in English. (Religion and Society, number 17.) The Hague: Mouton; distributed by Walter de Gruyter, New York. 1982. Pp. xi, 619. \$75.00.

The problem of secularization has figured prominently in the agenda of historians and sociologists of ideas since the days of Max Weber. Whether one argues that secularism demands a radical break with traditional values or has only poured old wine into new bottles, it remains perhaps the most distinctively "modern" aspect of modern thought.

Jean-Pierre Sironneau and Harry J. Ausmus are both firm exponents of the doctrine that secularization has rechanneled, not extinguished, the religious impulse. For Sironneau "the intent of the sacred" is a changeless constituent of the human condition. Secular systems of belief represent mere displacements or transformations of religious experience. For Ausmus, secularization is a "cover-up," a reworking in new language of the ancient hopes of religion. In short—for both scholars—the secular mind attacks institutional religion and theological dogma only in order to set up its own churches, its own creeds, and its own gods.

Sironneau makes the stronger, or at any rate the longer, case. Assembling his conceptual apparatus from the rich resources of Franco-German sociology and anthropology, he analyzes the "political religions" of the contemporary age, and in particular Nazism and Leninism-Stalinism. Such secular belief-systems exhibit all four dimensions of religious experience identified by Joachim Wach: myth, rite, communion, and faith. They are the functional equivalents, Sironneau contends, of the religions they sought to displace. But Sironneau does not let matters rest there. Dispensing finally with the value-free jargon of recent social science, he charges secular faiths with the sin of idolatry. Locked into worship of specific time-bound objects, they are poor substitutes indeed for the visions of otherness and transcendence available in the mythical structures of traditional religions. Modern man fools himself if he thinks he can live wholly within history: he needs a cosmic sacredness to rescue him "from the wanderings and from the prison of time" (p. 565).

Ausmus agrees that most secular systems of belief replace hope for eternity with hope for the future, but he declines enlistment in the fray on the side of presecular consciousness. His point of departure is what he calls "overt nihilism," derived in good measure from Arthur Schopenhauer. Like Schopenhauer, he sees the human condition as fundamentally tragic, characterized by endless suffering. The twofold function of religion has always been to proclaim this truth and then to hold out the promise

of salvation. The salvation never comes, the suffering never ends, but religion goes on. In the modern world, writes Ausmus, all that has happened is the replacement of one symbology by another. Salvation is still promised, but optimism has shifted from one imaginary world to another, to the world of "progress" and "science" and "liberation." Suffering continues unabated. Only Schopenhauer, Albert Camus in *The Myth of Sisyphus*, and of course Ausmus himself, see through the veil of illusion.

Clearly, Ausmus and Sironneau belong to different schools of thought. But they are united in their steadfast scorn of modernity, which is perhaps the real reason for their critique of secularization theory. They remind us, usefully, of the continuities in history and the extent to which secular belief recapitulates religious belief.

Yet in the final showing they may have missed the whole point of secularity. If a secular mind does in fact exist, its chief interest for historians lies in the extent to which it does not merely paraphrase the world view of traditional faith, but also injects something new and idiosyncratic into the flow of thought. Sironneau's view of the sacred and the agnostic gnosis of Ausmus's disbelief in even the possibility of human progress act as barriers to a full understanding of how secularity works in contemporary civilization. They are barriers, one suspects, even to self-understanding. Sironneau and Ausmus themselves are exhibits in evidence of the power of uniquely modern forms of secular consciousness. Their hypotheses may help to demythologize Hitler and Stalin, who, in any case, make easy targets. But such hypotheses are no help at all in explaining Sironneau and Ausmus.

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MORRIS BERMAN. *The Reenchantment of the World*. Ithaca: Cornell University Press. 1981. Pp. 357. Cloth \$34.50, paper \$8.95.

LOREN R. GRAHAM. *Between Science and Values*. New York: Columbia University Press. 1981. Pp. x, 449. \$19.95.

Loren R. Graham and Morris Berman share a sense that science and values have more to do with each other than is generally recognized. Graham's *Between Science and Values* has the modest aim of showing that various kinds of "links" between ideas about nature and ideas about what is good can be found in the work of many twentieth-century scientists. Graham is content to raise doubts about the validity of the classic notions of "value-free science" and "science-free values" and to counsel a cautious middle way in which we avoid both "the premature

attempt to explain cultural values exhaustively in terms of science" and "the attempt to define all science as intrinsically value-laden" (p. 382). Berman's *The Reenchantment of the World* makes exactly the second of these attempts and treats the dominant civilization of the West since 1600 as a monolithic and pernicious denial of the unity of fact and value that Berman believes to be inevitable, even in the lives of people who deny it. Berman's aim is as ambitious as Graham's is modest: Berman wants to place the scientific civilization of the modern West in the context of the entirety of human history and even of the natural history of the planet and to outline the "holistic" philosophy of nature (with its attendant "research programs") appropriate for the future. Graham and Berman agree that the dogmatic separation of fact from value is a mistake of relatively recent origin (Graham traces its important influence to the nineteenth century, Berman to the seventeenth), but the two authors assign almost antithetical meanings to it. For the incorrigibly sensible Graham, the mistake is a troubling and potentially dangerous bit of naiveté on the part of a civilization whose worth he does not question; for the frankly apocalyptic Berman, the mistake is a monstrous depredation against all earthly life and is, moreover, the crucial component in a singular cultural-economic-political system from which we ought to liberate ourselves in the interests of re-establishing our rapport with nature.

Graham's book is devoted primarily to the sequential study of prominent interpreters of twentieth-century science, especially Albert Einstein, Niels Bohr, Arthur Eddington, Werner Heisenberg, Henri Bergson, Jacques Monod, Konrad Lorenz, Tielhard de Chardin, B. F. Skinner, V. A. Fock, and E. O. Wilson. Graham also addresses eugenics and recent controversies in biomedical ethics. Although one concern of Graham's is to show the inadequacy of the "is-ought" distinction for understanding the actual work done by scientists in specific social and ideological contexts, the bulk of *Between Science and Values* outlines and analyzes what Graham's subjects have themselves said or implied about the issue of science's relation to values. Hence, Graham treats his subjects in their important capacity as philosophers or lay philosophers. Graham is at his best in explicating texts, and one can only wish that he had gone on to analyze other relevant, popular works by Henri Poincaré, Alfred North Whitehead, Pierre Duhem, Bertrand Russell, and John Dewey. Yet the direction of the book is not toward a fuller history of popular ideas about science and values; rather, its momentum is always toward a consideration of the problem of science and values *itself*. In his concluding remarks, and intermittently throughout, Graham urges us to transcend the crude extremes to which most of his subjects were prone: a "restric-

tionism" that declared science irrelevant to such obviously value-laden enterprises as religion and social philosophy, and an "expansionism" that expected science to help us decide how to live. The advice to get beyond the formulations of Graham's chosen subjects is certainly sound, but *Between Science and Values* would offer more to contemporary discussion had it addressed the actual arguments of thinkers less innocent, philosophically, than the likes of Eddington and Heisenberg, and had it demonstrated the inadequacy of contemporary moral philosophy and philosophy of science by translating into the terms of these disciplines the lessons that Graham believes can be learned from the history of science.

Berman's book is explicitly in the tradition of Theodore Roszak's *Where the Wasteland Ends* but is informed by a better knowledge of the history of science and by greater skepticism about the political and intellectual implications of recent enthusiasm for the occult. Berman judges the post-1600 civilization of the West uniquely unstable, destructive, and at odds with the structure of possibility presented to us by our planet. He idealizes the rest of human and natural history and defends alchemy, for example, even to the point of entertaining the possibility that medieval alchemists produced gold. Berman is remarkably oblivious to the violence of nature and to the lack of loving harmony in many human communities outside the perimeter of modern, scientific civilization. Berman's account of that civilization is more hostile than Jacob Bronowski's *Ascent of Man* was appreciative. The most useful part of *The Reenchantment of the World* is Berman's extensive exposition of the work of Gregory Bateson, whom Berman nominates as the new Descartes, articulator of the premises of a new epoch in which the unity of fact and value will be acknowledged and acted upon. This discussion of Bateson injects a note of intellectual seriousness in a book that too often sounds like the visionary young woman in the recent film, "Atlantic City," who was expert at foot-massage and did not believe in gravity.

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DOUGLASS C. NORTH. *Structure and Change in Economic History*. New York: W. W. Norton. 1981. Pp. xi, 228. \$19.95.

Douglass C. North, the doyen of U.S. economic historians, has given us a literate and wide-ranging study of structure and change in economic history. His book re-evaluates classical, neoclassical, and Marxian theories of economic change. He adds to this his own theory of hierarchical organization-

institutions based on market (contractual) relationships and economic constraints, which he finds crucial to long-term economic change. Hierarchies are central to the organization of production in modern economies and have a profound effect on economic performance and structure.

North's theory of institutions is multitiered. The determinants of hierarchical organization consist of a theory of property rights (incentives) and transaction costs (costs associated with human interaction—information costs, agency costs, costs of shirking, opportunism, and uncertainty), a theory of the state (which specifies and informs property rights), and a theory of ideology (which explains deviations from the rational maximizing behavior of the neoclassical school). Thus the theory of institutions explains why individuals do not always act in conformity with their own self-interest.

The institutional framework is then applied to a sketch of economic history. The history is based mainly on secondary sources and covers seven of the fifteen chapters in the volume. North finds that two great economic revolutions in history initiated radical secular change in institutional organization. The first revolution took ten millennia and was characterized by population growth, increasingly efficient types of economic organization, and growth in the size of states. Improvements in institutional organization in Egypt, Persia, Greece, and Rome led to lower transportation costs, regional specialization, and widening of the market, which resulted from growing security of property rights. Changes in the classical era are explained by changing relative prices and by ideology. Under feudalism and manorialism, economic change depended on military technology that made feudal lords obsolete as well as changes in relative prices that led to radical alterations in the system of property rights. Economic change during the Industrial Revolution was based on the growth in the size of the market and greater specialization and division of labor. This led to an increase in transaction costs that produced a more rational economic organization and lowered the cost of technological change.

The first economic revolution, according to North, witnessed the transition from common property rights, where there was little incentive to improve technology and learning, to exclusive property rights (codified by Roman law) that provided direct incentive to improve efficiency and productivity. It is this change in incentive, according to North, which explains the rapid progress of civilization during the last ten thousand years.

The second economic revolution (based on science and technology) came about through growth in the stock of new knowledge, capital intensive technology, change in economic institutions, and new organizational innovations, that is, more specializa-

tion, an increase in the costs of measurement and enforcement of rules and compliance procedures (due to the lengthening chain of production and distribution), and new organizational forms that reduced transaction costs. The modern state created property rights and produced the high living standards of the twentieth century. The second revolution also gave rise to a crisis in institutional reorganization, the managerial revolution, and a massive alienation of groups within the state because of the need for labor discipline, bureaucracy, team productivity, and so on. Thus, the need for an ideological superstructure (capitalism) to legitimate the rules of the game as well as to cope with the problem of externalities—unpriced benefits and costs.

Karl Mannheim, a great authority on ideology, would have called North a historical conservative. The book contains highly original insights into the evolution of institutional forms of organization. North's approach to history, however, is highly eclectic. Property-rights theory has considerable explanatory value when applied to seventeenth-century Holland, eighteenth-century England, or nineteenth-century America. It has less relevance for the Etruscans, precapitalistic economies in early modern Europe, or the centralized command economies of the twentieth century. It is fair to say that the institutionalists as well as the neoclassicists still need to learn more about individual behavior and social and economic processes.

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JAMES A. RAWLEY. *The Transatlantic Slave Trade: A History*. New York: W. W. Norton. 1981. Pp. xiv, 452. \$24.95.

The systematic study of the Atlantic slave trade, which began in the 1930s with investigations by French and North American scholars, has seen a major evolution in the past decade. The combined impact of the growth of African history as a discipline and the revolution in African and Afro-American political consciousness has focused scholarly attention on this subject as never before. The rise of new fields of historical concern such as demographic and quantitative social history have also finally provided the needed tools for analyzing this forced migration. The pace of research has been astonishing and, from being one of the least understood developments in modern history, the Atlantic slave trade is now well on its way to becoming one of the best studied subjects.

So rapid have been the advances in this field that we are now in possession of most of the basic statistics that can be recovered from almost all of the European-controlled trades and have reached a

broad area of consensus as well as developing new and important hypotheses for future research. Having established the crude rates of mortality, for example, the field has moved on to the causes of that mortality and its implications for African and New World health and demographic developments. Having established the basic economic structure and rates of profitability, there is now a lively debate on the larger social costs and nature of the market structure of the trade. All of this rather intense activity has already produced several volumes of conference papers and has made for some lively debate in the journals.

It is in this context that the first works of synthesis have begun to appear. The earliest such study incorporating the newer findings was carried out by the French scholar Hubert Deschamps in his general survey of the trade in 1971. To this effort can now be added the study in English by James A. Rawley. Although Rawley's is not the first general survey by a professional historian, as claimed by the author, this is one of the more comprehensive efforts to date and, with certain reservations, is most welcome.

To undertake such a study as Rawley proposes, an author should have good command of several European languages and their historical literatures. Such an author should also have a reasonable grasp of the basic issues in the newer branches of social and economic history. Unfortunately for the reader, Rawley does not adequately fulfill this rather difficult set of requirements. His work is based primarily on English sources with only a moderate sampling of the French literature. Even his English sources, while up-to-date on the trade in its later aspects, are deficient and dated for an analysis of the European background and the pre-eighteenth-century trade. Equally, the author, while doing a reasonable job in summarizing much of the economic and social findings of the works he studies, often cannot fully integrate them in the text. This may be due to his stress on a narrative style at the expense of careful analysis. Too often Rawley refuses to postulate clear hypotheses and obscures the issue with an excess of description or uses very imprecise wording as to the direction and relative importance of causal factors. Even the two general chapters on the middle passage and the economics of the trade suffer from this defect.

Aside from these general problems of style and sources, the author also ignores or misinterprets some issues that have been much discussed in recent studies. Thus the lively debate among economists as to the nature of the slave-trade market and the ultimate distribution of costs and benefits is not treated. He hardly deals with the crucial role of East Indian textiles in the analysis of either the local or international implications of the trade. His discussion of the slave mortality issue always stresses the

rates in comparison with white crews and whites in Africa. But for most purposes, the comparison should be with either contemporary immigrant and troop shipping or stable peasant populations. The rates are much lower than the older literature stressed, but they are still not even close to a normal mortality experience for nonmigrating young adults, and in the nineteenth century do not compare favorably with immigrant shipping. Rawley also stresses the older notion of American planter preference primarily influencing the origins and composition of the forced African migrants, to the exclusion of counter hypotheses in the recent literature. Finally, he comes close to a deterministic sickle-cell model for explaining the use of African slaves on American plantations, to the neglect of a serious analysis of comparative international labor costs and markets.

Where Rawley is most original is in the treatment of the English and North American slave trades. Not only does he summarize well the latest findings about these two trades, but he also adds some original research. In the former, his findings on the late eighteenth-century London and outport trades are quite important, and he has also provided much new material on the North American participation. Thus, whatever the book's limitations are in other areas, readers will find this a useful survey of the English trade and the best survey to date on the North American slave trade.

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CARL BOGGS. *The Impasse of European Communism*. Boulder, Colo.: Westview. 1982. Pp. xiii, 181. Cloth \$23.50, paper \$10.75.

JOSEPH V. FEMIA. *Gramsci's Political Thought: Hegemony, Consciousness, and the Revolutionary Process*. New York: Clarendon Press of Oxford University Press. 1981. Pp. ix, 303. \$24.95.

These books share a concern with the later, subtlest political reflections of Antonio Gramsci and the modern Italian Communist party's (PCI) claims to descend from them. Joseph V. Femia draws mainly on the "Notebooks" (1929-35) of Gramsci's last, incarcerated years and sets out to rescue his subject from beneath a paper tumulus of politically interested interpretation. Adopting a common-sense approach to exegesis, he succeeds to a remarkable degree. Discussion is organized around two initial chapters on Gramsci's notion of "hegemony," its sources, variants, ambiguities, and advantages. Following chapters discuss the revolutionary party as architect of socialist hegemony, the mix of libertarian and totalitarian elements in Gramsci's vision of socialist society, and how far the PCI is right in

claiming Gramsci's paternity for its present "road" (and what that may be).

In sculpting his Gramsci, Femia cuts energetically into the secondary literature that, reasonably enough, he classifies according to its political rather than intellectual premises. The figure who emerges, if not the most familiar depiction of Gramsci, is recognizably multisided: a politician whose larger thinking ran autonomously; a student and reluctant admirer of the resilience inherent in state and cultural forms; a man fascinated by the role of intellectuals in producing moral and cultural advantage; a theorist constrained by the bolshevism-in-retreat that he had done much to impose on the infant PCI; an enthusiast of theory and culture, rather than directives, for securing party cohesion in the "war of position"; the bender of philosophical Marxism to its relativist extreme, serviceable in the hands of his "organic intellectual" NCOs, but scientific in the eyes of those they lead.

Femia's "Evaluation and Conclusion" declares his interest in Gramsci as a beacon for the reinvigoration of a genuinely liberal Marxism. His lucid introduction warns of the twin dangers, in the study of political thinkers, of dissolving universal ideas into their immediate context, or of overly neat synthesis into a unitary body of thought. Femia's study errs, if at all, in the second direction. The primary research for the doctoral thesis on which it is based was completed before Valentino Gerratana's most thorough, chronological edition of the *Quaderni* (1975) appeared. With that to hand, perhaps a slightly fuller, more biographical exploration of, for example, relations between Gramsci's political and cultural reflections, or the impact of international politics on him, might have been possible. Nonetheless Femia's informed affection has yielded the best account of Gramsci's later political thinking available in English—a model of clarity that preserves all of its subject's major contradictions.

Carl Boggs, author of *Gramsci's Marxism* (1976), inserts Gramsci also into *The Impasse of European Communism*. This is, however, mainly concerned with the recent "Eurocommunist" evolution of the French, Spanish, and Italian Communist parties, drawing most of its material from the last. The book is organized as diagnosis-cum-critique, sometimes swerving into polemic rather than historical account. It is concerned most of all with theoretical statements by prominent Communists, secondly with their policies, and little with the daily activities or texture of Communist organizations. The central thesis (geopolitics set within a left Marxist framework) is that these have regressed toward classical social democracy: "Mediterranean Communism" promises latterly to undertake the "structural reform" of capitalism that socialist parties have attempted in northern Europe.

The proposition contains an obvious portion of truth and bears on a great number of interesting problems. Pursuit of these, however, is constrained both by the fact that Boggs's main frame of reference for evaluating Eurocommunism is his own view of what a (more revolutionary) Marxist movement ought to do, and by his indifference to national (never mind regional or local) differences within Eurocommunism. Of the two conventionally associated (but often conflicting) indices of Eurocommunism—pacific domestic politics and divergence from the USSR—Boggs takes only the first as important, thus not coming to grips with such questions as why the most pro-Soviet of his three parties should have docilely entered a "structural reforming" government, or why his Eurocommunist "troika," which began to emerge after the Soviet invasion of Czechoslovakia, was fractured by the occupation of Afghanistan.

Although of very different quality, these two books have considerable overlap. For example, both seek a contrasting measuring rod in Lenin—hardly a steady instrument for calibration. And neither convinces that Gramscian "hegemony" is fundamentally different from Leninism (only administered orally, rather than by injection, so to speak). Of their corresponding chapters on congruences between Gramsci and Eurocommunism, Femia's is much the richer. But more fruitful than disputes over Eurocommunism's hypothetical seeds in the thinker Gramsci (who never wavered in his view that communism's success was not to be achieved by sophisticated politics alone, but required capitalism's economic collapse) would be a study of its actual ones in, say, the politician Palmiro Togliatti, main leader of the Italian party from Gramsci's arrest in 1926, through a half-dozen major turns of Soviet policy, to his own death in bed at Yalta in 1964. The "historic compromise" was preceded, after all, by several years of deft "polycentrism." But Togliatti's (like Georges Marchais's) biography awaits excavation.

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OLGA A. NARKIEWICZ. *Marxism and the Reality of Power, 1919–1980*. New York: St. Martin's Press. 1981. Pp. 337. \$28.00.

Olga A. Narkiewicz has written a concise and chronological critical survey of Lenin's revision and application of Marxist theory after the victory of the Bolshevik revolution in Russia. She also follows skillfully the metamorphosis of Marxism in power into Marxism-Leninism in the USSR, as well as its further global splintering into numerous Marxist-Leninist variants like Stalinism, Trotskyism, Tito-

ism, Khrushchevism, Castroism, polycentrism, African socialism, Brezhnevism, and Eurocommunism.

This book provides the most rudimentary, if not the most sophisticated, links that enable us to put those numerous critical evaluations of Marxism into a more realistic perspective. As a study of Marxism-Leninism and its variants in power, it will also serve as a model for a badly needed overview for another, yet to be written, chronological and analytical study of Marxism as a political protest movement seeking to seize power from 1847 till 1919. Only by a better balance of macrostudies and microstudies of (1) Marxism as a political protest movement before World War I and (2) the many Marxist-Leninist variants since 1919, can we understand the real communist challenge and its threat to democratic, open societies, as well as the real and hidden opportunities for responding to them. Also, by focusing on the behavior of the communist system in power, this study illuminates another paradox in the communist conduct of foreign policy. Overemphasis on outdated Marxist ideology, excessive anti-Western propaganda, and Soviet pathological hatred of freedom (all the consequence of communist desire to remain in power permanently) made almost impossible a more rational and peaceful foreign policy of true "peaceful coexistence" with other non-communist and even some communist nations. Hence, Soviet foreign policy is often at odds with its trade policy. This contradiction disturbs global progress toward mutual interdependence and sharing of technological knowledge; it encourages revolutionary violence and terrorism; and it exacerbates the arms race, local ideological-economic warfare, and global divisiveness.

Parts 1 and 2 graphically show Lenin's mastery in turning Marx upside down while claiming to be the most faithful pupil in preserving the supremacy of internationalism over nationalism. Stalin's contribution to the Marxist-Leninist model was his equally skillful manipulation of the international communist movement, modifying Lenin's theory and practice and blatantly incorporating Great Russian expansionist nationalism into the Soviet system while demanding subservience of foreign communists, through the Comintern (1919-43) and the Cominform (1947-56), to the national interest of the USSR.

Three remaining parts shed light on the post-Stalinist era. Internal crises of the USSR (destalinization, coups d'états—the execution of Beria, the removal of Marshal Zhukov, and the Malenkov-Molotov purge as well as the purge of Khrushchev) and greater desire for independence from Moscow (the Czechoslovak and Polish crises; renewed quarrels with Tito; and new splits with Albania and especially Red China, reinforced by Togliatti's polycentrism and its later Eurocommunist variants) frag-

ment and weaken Soviet domination over the international communist movement. The emergence of various regional, pluralistic communisms adds new challenge and an opportunity for communists in power and communists in opposition. Diversified regional, national, and neo-Marxist tendencies produce "competitive" or "national" communism and often a merger of the two. Varieties of peaceful and violent neo-Marxist approaches, in addition to revised official Soviet, Chinese, and Yugoslav ones, complicate our attempts at classification and predictions about the direction in which communism is moving.

Following the development and spread of communism over seven decades, Narkiewicz answers "who are the revolutionaries?" by classifying them as major revisionists of Marxism. Bernstein was the first major revisionist. Lenin was the second, since he created a monolithic, elitist party dominated by intellectuals instead of a mass workers' party as Marx had envisioned. Yugoslav and, later, Chinese communist revisionists openly proclaimed "the national road to socialism." The Latin American or "Hispanic" brand of Marxism began with a purer Marxist-Leninist-Castroist variant in order to grow, according to Narkiewicz, into a Caribbean alliance of "revolutionary peasantry, liberal intellectuals, and progressive clergy."

What is left of Marxism? Quite a lot when it comes to strategy for gaining power; very little when it comes to pure Marxist theory. Yet, despite all revisions, Marxism remains the most potent political protest movement of the contemporary world, most successful in those Third World countries where there is the greatest socioeconomic inequality and least personal freedom and dignity. As such, Marxism in its various forms is alive and well; as a unified theory of the nineteenth century, it has died many times over. Among its pallbearers we can include Bernstein, Lenin, Stalin, Tito, Mao, Togliatti, and Castro. While burying it, all those leaders also sanctified their own versions of Marxism.

By skillful manipulation of Marxist mythology, communist parties in power (and their leaders) thus gained the most coveted prize sought by all modern political parties in both open and closed societies—legitimacy for the indefinite political reign.

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ROY DOUGLAS. *New Alliances, 1940-41*. New York: St. Martin's Press. 1982. Pp. 154. \$22.50.

A typical Roy Douglas book, in the same genre as his *In the Year of Munich* and *The Advent of War, 1939-40*, this little volume traces the deep, irreversible

shift in the balance of world forces from May 1940 to the end of 1941, as the Second World War assumed proportions quite unrecognizably beyond those that attended its outbreak. The story is told from the vantage point of Britain, and largely British sources are utilized. Much of it is generally familiar, and the merits of the work lie mainly in its convenient brevity, its sharpness of organization and style, and the crispness of the author's judgments. The first and third of these are weaknesses as well, for there are places where Douglas writes with assurance about decision making in countries other than Britain (the United States and Russia, for example, as well as some small European states) without providing sufficient analysis of his own or demonstrating familiarity with pertinent secondary literature. While his judgments are, in the opinion of this reviewer, generally apt, one is left with the feeling that certain things are being assumed. On balance, then, the work will be of greater use to students and general readers than to scholars, save those of British foreign policy.

Douglas's treatment of the British dilemma over how to deal with France, both before and after its capitulation, shows sympathy for both sides. His explanation of why the British fought on, despite some potent reasons for dealing with Hitler, is the realization that they *could* do it—a realization that emerged from the rescue at Dunkirk, the strong performance of the Royal Air Force against initial German air attacks on England, and the expectation of gradually increasing aid from the United States. Indeed, by late summer 1940 British morale was so remarkable that no government would have found it politically possible to make peace with Germany. Nor did the stalemate that developed in the war during the first half of 1941 materially change this. The Nazi-Soviet Pact is depicted as a bargain between thieves, and little sympathy is shown for Soviet policy generally. Russia's forced entry into the war necessitated wrenching adjustments by other nations, and it was clear from the beginning—and accepted both by Britain and America—that the issues at stake among the strange allies, and between Russia and Poland in particular, would be determined by the fortunes of war, not by any sort of moral principle. Attlee comes off well for rejecting Stalin's early demands, Eden poorly. In the Pacific, Churchill was determined to act together with Roosevelt to ensure that further Japanese encroachment would bring the United States into the war. The British were not completely opposed to compromise with Tokyo, but fear lest the United States disengage with Japan and leave Britain to its fate led to a harsher posture than might otherwise have prevailed. Pearl Harbor radically altered the whole outlook, and the German decision to declare war on the United States sealed the fate of Hitler's Europe.

Thus run some of the more important interpretive themes in Douglas's book.

There is also comment on the origins of the Cold War. Profound disagreements among the Big Three lay in a fundamental difference of objectives in the war, not simply in later wartime decisions, Douglas asserts, and a postwar showdown was unavoidable. The wisdom of the Western Allies in shipping to Russia supplies that they desperately needed themselves, when this apparent difference was so obvious, is bluntly questioned. "Today . . . the world is still paying the price of failure to face squarely the implications of what was said in Moscow in December 1941 [Eden-Stalin conversations on war aims]" (p. 135). But there is no attempt to assess the impact had Russia in fact collapsed.

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WALTER LIPGENS. *A History of European Integration. Volume 1, 1945–1947: The Formation of the European Unity Movement*. With contributions by WILFRIED LOTH and ALAN MILWARD. Translated by P. S. FALLA and A. J. RYDER. Oxford: Clarendon Press of Oxford University Press. 1982. Pp. xvi, 723. \$98.00.

This first of a multivolume effort at depicting and interpreting the historical process of European integration (read "merging of sovereignty") formally covers just two years. Walter Lipgens has done that intelligently and thoroughly and given the reader much more. His full historical backdrop includes chapters on interwar European union failures, resistance-based and wartime regionalist activities in London, superpower points of convergence and divergence about the Continent, and "Europe's self-awareness." But nearly six hundred pages are nevertheless devoted to 1945–47, and that rationale is sound. To comprehend the European unity movement is to emphasize the forces that gave life to the entire postwar movement until sufficient governmental interest was aroused. The volume reviewed here therefore explores in depth the various associations, pressure groups, and individuals who helped to carry the integrationist messages to politicians, parties, and power elites. Thus, this is the tale before the issue was one of practical politics. The forthcoming volume will focus on 1948–50 and the interaction of parties and governments.

Lipgens has written a comprehensive history of supranational interest groups in postwar Europe. He traces their emergence from the ashes of wartime Allied cooperation and the developing East-West conflict. He has created a new primer for many contemporary historians who have been dependent on viewing the postwar world through Soviet-American historiographical glasses. "It is [a

mistake] to suppose that the movement for European union was merely a product of the later conflict between East and West, of Soviet threats and American inducements; it had long existed as a purely European reaction to the catastrophe into which the continent was plunged by the system of nation states" (p. 685). The breakdown of cooperation among the big powers is, however, tightly related to the breakthrough of federalist concepts and groups and the notion of Europe as a "Third Force." Survival of the now less powerful European states was only possible within an economic and political union, with regional institutions that pooled separate national sovereignties. Lipgens drives home the early strength of these federalist ideas and the developing perception that European independence from the two world powers demanded the deliberate engineering of institutions to force nations into tighter and unbreakable supranational molds. The multiplicity of nongovernmental associations that he describes and assesses are the heart of the work, especially the UEF (Union Européenne des Fédéralistes). Although it is evident that these bands of integrationists were the "pioneers" and "sustainers" of a revolutionary movement, Lipgens correctly denotes the divergencies within various national groupings that presaged later regional conflicts.

Yet it is in the political philosophy of these federalist writers, the elites of The Hague, Zurich, Luxembourg, and Montreaux congresses, and in detailing their rhetoric and propaganda that this monograph is *new* history. This is the first truly comprehensive view available of the new breed of "Europeans" and their vehicles of change. The volume is an archival gold mine, with personal, institutional, and state depositories elaborately and intelligently cited and utilized. The programs of the pro-European associations and the ideas of major "Europeanists" are so central to the book that it is in fact an intellectual history contained within a political and diplomatic study.

Although social scientists have made innumerable specialized studies of these years and issues, historians have devoted surprisingly little attention to the systematic investigation of the postwar "New Europe." This historian has cut his way through enormous detail and demonstrated throughout an amazing ability to piece together the essential factors of that story. His grasp and wide use of source materials are remarkable. In a substantial and exceptionally able study, Lipgens has performed the difficult and delicate task of presenting an intricate problem in a commendable manner and with a vigorous style. The number of organizations and national subunits explored required the author to attack a massive tangle of evidence, and he has reduced it to order. His general conclusions, particularly when

they concern the interconnections between the European unity movements and the realities of global powers, are not obtrusively or forcibly imposed on his detailed narrative and reasonable interpretations. It is a valuable piece of recent history, an admirable example of its kind. I look forward to the next volume and the diligent labors and sagacious insights of Lipgens.

It should be noted that (1) Alan Milward and Wilfried Loth contribute helpful chapters and (2) the translation by P. S. Falla and A. J. Ryder is a professional job with only a few sections of contrived and awkward language. The price in U.S. dollars, however, is not justifiable and *shameful*. Even institutional buying may not be possible and certainly private purchases will be few.

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EVAN LUARD. *A History of the United Nations*. Volume 1, *The Years of Western Domination, 1945–1955*. New York: St. Martin's Press. 1982. Pp. viii, 404. \$27.50.

This is the first installment of a multivolume enterprise undertaken by an Oxford scholar-politician well known for a variety of useful books on modern international affairs, and for his devotion to the internationalist cause. That devotion is clearly to be felt in these pages, which offer a commentary and judgment (from a recognizable liberal point of view) on the first decade of the United Nations as well as a "straight" history of its origins, formation, and activities. But scholarship is here also, scrupulous with regard for evidence and sensitive to all matters of nuance, supposition, and controversy. An adequate set of chapter headings and a good index make it conveniently usable. All in all, it is well set up to serve as a basic text for anyone who wants to know about the UN and its place in the history of our times.

Such a feat of concision is not to be achieved without strong-minded decisions about how to do it and what to leave out. Evan Luard comes clean at the outset: "it is concerned almost exclusively with the central responsibility of the organization, the maintenance of peace and security," and it generally bypasses the UN's work in the fields of economics and human rights, together with all the specialized agencies "which are part of the UN 'system' but not of the UN itself, having separate memberships and assemblies" (p. vii). So half of the chapters bear territorial titles: Greece, the Korean War, Guatemala, and so on. The rest refer to its institutional foundations, the dashing of its original plans for a security system (the territorial narratives that follow show how trial and error in a cold climate gradually

evolved alternatives), and the particular problems presented by disarmament, membership, and the Secretary General and the Secretariat.

Luard writes with authority, centering his analyses always on the debates in the Security Council and General Assembly and the politicking that went on around them. A small limitation to the usefulness of his book for higher-level scholars is its lack of scholarly apparatus: few sources are indicated (never any of the debates), there is no bibliography, and no such concessions to human frailty as chronological tables or maps. But the index is good and the prose is lucid, measured, and impressive.

A theme Luard likes to hammer home is the nemesis forged by the United States in moving as much business and authority as possible from the Security Council to the General Assembly, to get it away from the Soviet veto. In course of time, the U.S. hoped, the General Assembly was bound to change its tune. Luard certainly cannot be called soft on the Soviet Union, but he goes to some pains to explain the USSR's reliance on the veto as its only means of keeping the UN reasonably close to the original conception of the organization as one in which the great powers were to call the shots, with the proviso—so the USSR understood—that unless the great powers could agree on a policy no shots were to be called. He clearly, however quietly, believes that American-led use of the UN for Cold War polemics and propaganda was a disaster for the organization's purpose of establishing a new and realistic international security system. His other recurrent major criticism is that again and again, whether due to Cold War inclinations or not, the UN failed to promote conciliations and negotiations toward compromise settlements, allowing instead the hardening of attitudes in unrestrained polemical debate.

Luard's next volumes will be pleasurably awaited. May they not be long delayed!

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ANCIENT

A. W. GOMME *et al.* *A Historical Commentary on Thucydides*. Volume 5, *Book VIII*. Oxford: Clarendon Press of Oxford University Press. 1981. Pp. xv, 502. \$74.00.

Thirty-seven years separate A. W. Gomme's publication of the first volume of this commentary and Antony Andrewes's final volume on book 8. Reviews of earlier volumes hailed the work as "brilliant" and "indispensable." After Gomme's death the continuation by Andrewes and K. J. Dover maintained that

exceptional standard, and the new volume fulfills the high expectations raised by its predecessors. The commentary provides a full and precise treatment of events, persons, and geography, with copious references to recent historical literature. Many notes run several tightly argued pages, and some longer. A special monograph is devoted to the sources for the revolution of the Four Hundred (pp. 184–256), including a full commentary to *Athenaion Politeia*, 29–33 (pp. 212–40). Two long appendixes by Andrewes and Dover respectively finally redeem Gomme's promise in volume 1 to set forth his ideas on the composition question. A chronological table for 421–11 B.C. and indexes to all five volumes complete the book.

Andrewes's task of historical explanation is the harder because Thucydides frequently omits in book 8 what would seem to be important material. Andrewes cites extensively non-Thucydidean evidence, especially inscriptions, and his cautious evaluation of the combined sources is exemplary. His technique is to lay out the evidence and its interpretations by scholars of the last hundred years and succinctly state his own judgment, expressing precisely the degree of confidence he feels in the conclusion. Like Gomme, Andrewes treats issues not actually raised by Thucydides, if the Thucydidean passage has become relevant to modern discussion: for example, whether under the Five Thousand other citizens were excluded from all aspects of government (pp. 323–28).

Dover's appendix on strata of composition (pp. 384–444) is a solid and sensitive synthesis, defining the boundaries of discussion of an immensely complex subject, and greatly expanded from the section devoted to composition in his 1973 pamphlet, *Thucydides*. After noting the difficulty of the problem, Dover uses the formal variations of year-end formulas and the presence or absence of speeches and documents to distinguish the five major sections of the work. He sees a change in Thucydides' methods in writing the speeches, following Pohlenz in arguing that Thucydides intended originally to put into practice 1.22.1, but later departed from that method without reformulating his statement (pp. 396–97). He does not, however, see any simple and convenient touchstone for judging the accuracy of the speeches or their date of composition. Examining the causes of the war, Dover rightly rejects the temptation to make a dichotomy between apparent and real causes: in book 1 as we have it, the two reasons merge into one. "It could fairly be said that the 'real reason' for the war, that Sparta 'had' to fight against Athenian power, was illustrated by Kerkyra's embassy to Athens" (p. 423).

If there is an overall theme to this volume, it is the incompleteness of book 8 and the insight this gives into Thucydides' method of composition. Andrewes

in appendix 1 (pp. 361–83) reviews the evidence for incompleteness throughout the history, section by section, under the headings of military narrative, chronological framework, and consistency of viewpoint. There is no doubt that in all areas book 8 falls markedly short of the general standard, book 5. 14–116 somewhat less so. Outside of the appendix, Andrewes frequently comments on the problems raised by a specific passage, such as the absence of direct speeches and the flashback describing Alcibiades' intrigues. Most often he finds the reason for the difficulty in Thucydides' decision to await better information, or his having added new material without sufficiently integrating it with the old. More, however, is involved in writing history, especially Thucydidean history, than finding good sources and interpolating new information into old drafts. By showing us Thucydides' history in progress, book 8 allows us to recognize more fully the unified historical perspective that has been imposed in the earlier books (for example, the decision in book 1 to de-emphasize the Megarian decree).

In book 8, Thucydides faced a task perhaps more difficult than he had ever faced before, and it is not surprising that he ran into difficulties of interpretation and presentation. In particular, he was forced to re-examine the role of Persia in the war. Andrewes, of course, is familiar with this problem, having dealt with the Persian omissions in Thucydides some twenty years ago (*Historia* 10 [1961]: 1–18). But I believe that he underrates the effect of Persia's decisive entrance into the war on Thucydides' history. As historian, Thucydides attempts to fit every particular into his conceptual framework. It is apparent, however, that until well into the Ionian War, Thucydides thought that the role of Persia in his history would be negligible. Not only did he exclude major Persian matters, but throughout he presented the Athenian empire as completely free of the Persian threat. Neither Nicias before the Athenian assembly (see especially 6.10) nor Alcibiades at Sparta consider Persia a danger to Athens. The whole of Thucydides' history as we have it is built on the conflict of two "superpowers," Athens and Sparta. To integrate into the narrative Persia's deciding influence on the war would need not a few notes here and there, as Andrewes suggests (for example, p. 369), but a fundamental rethinking of the nature and meaning of the war. If Tissaphernes was powerful enough to wear down both parties (see for example 8.46.4, 56.2), then the meaning of the war was no longer to be found in the decisions of Sparta and Athens alone, and the chaos of events threatened to escape the order so brilliantly imposed in the earlier books. Perhaps not lack of information or time, but the need to rethink *de novo* his interpretation of the war kept Thucydides from completing his history.

But one can hardly complain that a commentary originally projected for three volumes and now grown to five has not covered enough. Andrewes has superbly fulfilled the task begun a generation ago, to clarify what the author said, its historical implications, and its relation to our other sources. All ancient historians and students of the fifth century will want the full set on their shelves.

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PETER SIEWERT. *Die Trittyen Attikas und die Heeresreform des Kleisthenes*. (Vestigia, number 33.) Munich: C. H. Beck'sche Verlagsbuchhandlung, for the Kommission für Alte Geschichte und Epigraphik des Deutschen Archäologischen Instituts, Berlin. 1982. Pp. xviii, 183. DM 82.

This book is much more important than its title would lead one to believe, because Peter Siewert's purpose is to explain what Kleisthenes had in mind when he carried out his sweeping reorganization of the traditional tribes of Athens, creating ten new ones subdivided into thirds called *trittyes*, one each drawn from the urban, coastal, and inland regions of the country.

Siewert's thesis is that Kleisthenes' allotment of demes to the *trittyes* was usually governed by geography. He reargues H. Sauppe's old (1846) case that in the city demes were assigned to the urban *trittyes* in official tribal order proceeding from the two Agryles (given Erechtheis) at the southeast corner of the city counterclockwise to Alopeke (given Antiochis) due south of town. He then shows that topography was important elsewhere. W. E. Thompson's study of the lists of councillors revealed that demes were usually allotted to *trittyes* to make nearly equal units of seventeen, seventeen, and sixteen *Bouleutai* for each tribe. Exceptions to this rule were caused by oversized demes like Acharnai, whose twenty-two councillors alone made up Inland-Oineis. Alongside Thompson's scheme, Siewert deploys a striking fact. In a majority of cases demes were assigned to inland or coastal *trittyes* because they were strung out along the main roads. Thus, the demes of Inland-Akamantis stretched from Prospalta near Sounion northwest along a route that ran by Hagnous and Sphetos to Kykinna, and from there around the southern edge of Mt. Hymettos into the city. Siewert thinks that the *trittyes* that were not organized in this way were the result of having to match up demes of widely varying size. Kydathenaion on the northeast slope of the Akropolis had the considerable number of twelve *Bouleutai*; it was joined to Probalinthos south of

Marathon with five to make Urban-Pandionis with seventeen.

From all this emerges Siewert's principal finding. The main purpose of Kleisthenes' reorganization was to facilitate the quick mobilization of the army. Each *trittys* was to contribute one *lochos* of three hundred men (the nine thousand hoplites of Marathon divided by thirty), which could reach the city usually by the fastest route in a formed body ready for action. The *trittyes* would assemble in the Agora, where the Athenian roads converged, probably near the Altar of the Twelve Gods, from which distances were measured (Herodotus 2.7; *IG* II² 2640).

This interpretation is doubtless correct. It will force a change in the views advanced by scholars like A. Andrewes and D. M. Lewis, who have thought that the desire to break up concentrations of aristocratic power governed assignment of demes to *trittyes*.

Not everyone, however, will want to agree that Kleisthenes had a single motive. It is possible that a desire to reduce the power of certain factions was a secondary matter. In eastern Attika, the stronghold of the Peisistratidai, thinking still impossible to fathom influenced the assignment of demes, as Siewert admits. He also maintains that rapid assembly of the hoplites was wanted only to make the rise of new tyrannies difficult and thus to smooth the way for the exercise of power by established families like his own. It is hard to agree with that, too, because when Kleisthenes was active, Athens was the rival of formidable enemies—Aigina, Boiotia and its Euboian allies, and Sparta and its league. Defense (or aggression) against these powers may have seemed more important than guarding against dictators.

Siewert's book is carefully argued and based on the most recent research in several fields. The book includes two excellent road maps of Attika and D. Traill's political map, here with the demes indicated by size and in color by tribe. A fourth map superposes the demes on the road net. The index is detailed. The German is not difficult.

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JAY BREGMAN. *Synesius of Cyrene: Philosopher-Bishop*. (Transformation of the Classical Heritage, number 2.) Berkeley and Los Angeles: University of California Press. 1982. Pp. xi, 206. \$25.00.

Synesius of Cyrene, just before he became bishop of Ptolemais in Libya early in the fifth century, said he would enter the priesthood only if he could (1) believe that the soul is immortal by nature, (2) refuse to believe that the world will one day be destroyed, and (3) maintain his double standard, especially in

relation to resurrection, of philosophical truth for the good of his own soul and mythological falsehood for the sake of the public. Many early Christians had these and similar difficulties with orthodoxy. What distinguishes Synesius, according to Jay Bregman, is the combination of so many reservations, and especially the overall impression Synesius gives of looking steadily to philosophy, not to Christian revelation, for his salvation.

Patristics scholars usually locate Synesius at one end of the spectrum of Christian Platonists. Bregman believes the method begs the question: "The traditional view of the relationship between Hellenic thought and Christian doctrine does not provide an adequate framework for an interpretation of Synesius' religious position" (p. 9). Bregman's basic argument, that Synesius's "acceptance of Christianity was provisional and remained secondary to his commitment to Neoplatonism" (p. 5), is forceful, though the force is blunted by the frequency with which he asserts the argument. To organize chronologically the story of a figure who you claim did not change very much throughout his lifetime is not the best strategy.

Bregman's supplementary arguments are often intriguing. He suggests that Synesius, by inventing the office of "philosopher-bishop," transposes Plato's "philosopher-king" into a key appropriate for an empire in which Christianity is clearly the wave of the future. Synesius appears as one who sympathized with Julian and Symmachus but escaped their hopelessly outdated linkage of classical philosophy with traditional ritual and theurgy. "He would have to forge an individual path in a world that respected Hellenism, but was no longer pagan" (p. 49).

Bregman makes some astute observations on the appropriation by Christian monks of the philosophic ideal. The example of monks whom Synesius early in his life treated with mild contempt ("where the Greek employs *paideia*, the barbarian ascetic weaves baskets," p. 132), may eventually have been a spoonful of sugar that helped Synesius get the medicine of Christian doctrine down his throat. A late letter implies envy of a friend who, having become a monk, "has attained serenity and achieved the goal [of the contemplative life] which Synesius has been seeking for so long." Synesius as bishop "even helped to found a cloister" (p. 171).

Synesius of Cyrene is a worthwhile contribution to study of "the transformation of the classical heritage." Even after Bregman's close study, however, Synesius remains elusive. Is he really a "syncretist" (see especially p. 144)? Bregman frequently calls him a Neoplatonist or, more generally, a philosopher, and once "simply an honest man" (p. 160). Synesius was able to synthesize a lot of things, but much synthesis does not necessarily make a syncretist. Bregman declares that Synesius was never a

Christian and never intended to be one (p. 11). I am not entirely persuaded that the traditional view of Synesius is misleading, for there was still a very wide range of identifiably Christian options. But Bregman has set me to thinking.

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MAURICE SARTRE. *Trois études sur l'Arabie romaine et byzantine*. (Collection Latomus, number 178.) Brussels: Latomus. 1982. Pp. 226. 1,100 F.

This is an important book for those interested in how Rome and Byzantium ruled the province of Arabia, southeast of Palestine and Syria, where unruly nomadic tribes represented a threat to imperial authority. It is composed of three complementary chapters. The first sketches changes in borders of Roman Arabia from A.D. 106, when it was formed, until the Arab conquest five centuries later. The second discusses what is known of imperial officials who governed it. The third sketches what is known of the Arab nomadic tribes who lived along its borders.

What emerges is a somewhat different picture from that most historians have long accepted. For instance, in the early imperial years Rome controlled much desert territory and caravan routes that led deep into the Hejaz and the northern Arabian peninsula. After the mid-third century its power receded toward Palestine and Syria proper. Rome also divided its Arabian province into two parts, one centering in the Sinai and areas east of the Dead Sea, essentially the old Nabatean kingdom, and a second composed of areas south of Damascus and east of the Jordan, centering in Bostra.

Secondly Maurice Sartre summarizes changes in the officials who ruled the province. At first the governors were senators. Then in 262 they began to be chosen from the equestrian class, who by the fourth century also commanded Roman armed forces. This continued until Justinian separated military and civil functions perhaps because this had become a particularly sensitive frontier during his reign.

Third and most important is the discussion of the nomadic tribes who lived along the borders of the province. At first the dominant tribes were the Nabateans in the south and the Saifites to the east. Then as Christianity spread into the desert new tribal confederacies appeared of which the Kindans and the Salihens were the most important. These confederacies were ruled by kings whom the Romans called *phylarchs* and whom they often subsidized as a way of keeping Arabia peaceful, though such policies were never totally successful and such confederacies remained essentially unstable. Finally

in the sixth century the Ghassanids became Rome's most important ally although their power was seldom exercised in the Sinai or the area impinging on the Gulf of Aqaba, which explains why they were unable to protect frontiers effectively when, after A.D. 630, large-scale Muslim invading armies reached Palestine and Syria.

In short Rome's effort to use subsidized tribal auxiliaries ruled by kings or *phylarchs* worked no better here than it had earlier when they used a similar system in attempting to hold their Rhine-Danube frontier. Its failure made both earlier Sassanian attacks and later Arab ones in the seventh century successful and ultimately helped bring about a new era of history.

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MEDIEVAL

CAROLINE WALKER BYNUM. *Jesus as Mother: Studies in the Spirituality of the High Middle Ages*. Berkeley and Los Angeles: University of California Press. Pp. xiv, 279. \$28.50.

Taken from the *Harvard Theological Review*, *Medievalia et Humanistica*, and the *Journal of Ecclesiastical History* and enlarged and completed by the addition of a chapter, *Jesus as Mother* nevertheless constitutes a whole book. Methodological and substantive concerns that are enmeshed in the introduction and the epilogue, as well as in the five essays lend uniformity to this volume.

In the spiritual realm 1050–1250 was a period of equilibrium. Herrad of Hohenbourg replaced the older equation of the religious person with soldiers, lay women, clerks, nuns, recluses, and hermits. Such an equation did not represent the predilection of one individual but of educated people at large, for they depicted humanity in similar terms when attempting to see the spiritual substance it possessed.

Within five chapters Caroline Walker Bynum can only touch on a few topics that seem important. The first chapter deals with the difference between regular canons and monks, pointing to their duty to preach outside the community of the regular canons. The second chapter concerns love for the community, for which the individual learned to feel a compassion that was deeply affective and sensual. The third chapter treats the twelfth-century discovery of the individual and emphasizes that the discovery was made within oneself in the image of God: that is, the discovery of one essence that is the divine. Thus, Bynum rejects the interpretation of Morris, Brown, Radding, and Hanning, who claim

that 1050–1250 saw an awareness of the self similar to that attributed to the Italian Renaissance by Jacob Burckhardt.

Only the fourth essay has “Jesus as Mother” as a topic. The Cistercians borrowed from Anselm of Canterbury the idea that abbots nursed their children like Christ and God. Devotion to Mary and the female saints increased in importance, and feminization of the language occurred. Although the later fourteenth-century thinkers placed more emphasis on Christ’s suffering, the twelfth-century Cistercians tried to temper the image of the autocratic father with that of the mother.

The last chapter, the only one not previously published, addresses the question of how three outstanding women of the thirteenth-century monastery of Helfta, strongly influenced by Cistercian and Dominican customs, were under the scrutiny of God. The youngest one, Gertrude the Great, is treated first, followed by Mechtild of Hackeborn and Mechtild of Magdeburg, a full generation older than Gertrude. The writings of Gertrude and Mechtild of Hackeborn are quite similar. Gertrude sees God the Father as just, and Christ is conceived of as mother and father, both stern and comforting. The difference between Mechtild of Hackeborn and Gertrude is that Gertrude’s Christ is more accessible; the stern side of him does not appear. Mechtild of Magdeburg (who entered the convent in 1270 when she was twenty-three years old, as opposed to her two sisters, who entered as children) views God the Father as a lover and Christ as the expiator of mankind. It seems at times that she writes of continuous, sinless humanity; at other times, she posits a hell composed only of men and princesses. She feels a greater sense of responsibility for the whole church than the other two. These three nuns, though they did not hold any offices, were counselors and advisers to their sisters. Their writings were perused with interest by the laity and the clergy, but the *Flowing Light of the Godhead*, with its doctrines of damnation, created an uneasiness in women’s theology.

If its title read, more appropriately, *Studies in the Spirituality of the High Middle Ages: Jesus as Mother*, Bynum’s book would be a volume of significance, picked with great interest from the shelves of scholars.

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ITHAMAR GRUENWALD. *Apocalyptic and Merkavah Mysticism*. (Institutum Iudaicum, Tübingen, Arbeiten zur Geschichte des Antiken Judentums und des Urchristentums, number 14.) Leiden: E. J. Brill. 1980. Pp. xi, 251. f. 80.

DAVID J. HALPERIN. *The Merkavah in Rabbinic Literature*. (American Oriental Series, volume 62.) New Haven, Conn.: American Oriental Society. 1980. Pp. xi, 212. \$14.00.

The earliest of the major trends in Jewish mysticism, which the late and great Israeli scholar Gershom Scholem defined and analyzed, is usually called “merkavah mysticism” because it is associated with Ezekiel’s imagery of a chariot, which postbiblical rabbinic sources refer to as *merkavah*. Both of these new studies reassess Scholem’s pioneering reconstruction of the character and meaning of *merkavah* mysticism and also address the larger question of how mystical ancient Judaism was. Both writers are intimately familiar with the pertinent passages from apocalyptic writings, tannaitic (Mishnah, Tosefta, *baraitot*) and amoraic (Talmuds) rabbinic sources, as well as with the later corpus of texts about visionary ascents through heavenly palaces (Hebrew: *hekhalot*) which define *hekhalot* mysticism. Yet these studies end up with remarkably divergent conclusions due in part to differing methodologies and in part to different degrees of scholarly independence from Scholem’s perspective.

Ithamar Gruenwald’s book is a sweeping reaffirmation of Scholem’s main thesis that the rabbinic sources of ancient Judaism attest to an ecstatic mystical praxis and that a continuous tradition of *merkavah* mysticism exists from the early apocalyptic, through rabbinic sources, and culminating in the *hekhalot* texts. Eschewing a form-critical approach as premature without critical editions of the texts, Gruenwald offers instead a thematic “comprehensive survey of the Merkavah tradition” (p. viii). Part 1 includes chapters on each of the main types of sources; part 2 provides more detailed expositions of the longer *hekhalot* texts. The book also includes two appendixes by the eminent rabbinic scholar, Saul Lieberman.

Gruenwald defines *merkavah* mysticism as descriptions of experiences of heavenly ascensions and of visions of God and angels, and he analyzes allusions to these and other related motifs in all of the texts he reviews. His comparisons of similar themes in Jewish, Gnostic, and early Christian sources provide an important introduction to the contours of the mystical elements in these religious traditions in late antiquity.

Unfortunately, there are serious flaws in the way Gruenwald has proceeded. Although he is aware that the presence of the literary motif of heavenly visions is not the same as evidence for actual ecstatic experiences (p. 63), he fails to apply this crucial methodological distinction and consistently equates them. Following Scholem, he also interprets the tannaitic and amoraic rabbinic sources about *merkavah* speculation in light of the later *hekhalot*

texts that explicitly deal with visionary ascents in an experiential context, even though he knows that the earlier rabbinic ones are almost exclusively exegetical in character (pp. 77–78, 82). But this tack serves only to reiterate, rather than to reexamine anew, the existing scholarly consensus. Gruenwald's own analysis indicates the deficiencies of Scholem's approach, but he draws back from the logic of his own argumentation apparently because of his methodological confusion and his unwillingness to question Scholem's assumptions. It is especially unfortunate that a book that offers readers a guide to all of the pertinent texts of the ancient Jewish mystical tradition has a woefully inadequate subject index in which almost none of the titles of the texts analyzed is mentioned, nor are there indexes to biblical and postbiblical sources. There is also no bibliography.

In contrast to Gruenwald's comprehensive surveyor's map of the entire textual terrain, David J. Halperin provides an archaeological cross section of only the multilayered tannaitic and amoraic sources but achieves impressive new results. By applying rigorous form- and redaction-critical methodologies to the texts, including all manuscript readings, and by adopting a posture of cautious skepticism towards the dominant scholarly consensus about the interrelatedness of the different corpora, Halperin concludes, against Scholem, that: (1) Palestinian rabbinic sources (Mishnah, Tosefta, Palestinian Talmud) understand *merkavah* speculation to be the exegesis of Ezekiel 1 and 10, not mystical praxis; (2) the only evidence in rabbinic sources that *merkavah* activity involves experiences of visionary ascents is found in the Babylonian Talmud, which suggests a Babylonian, not a Palestinian, provenance for such mysticism when it does appear; and (3) since rabbinic sources do not attest to any mystical praxis, Scholem's construct of a continuous tradition of *merkavah* mysticism is untenable.

In addition to his critique, Halperin also tries to reconstruct the historical reality that is presupposed by the warnings in Mishnah about the dangers attending *merkavah* speculations if the latter did not involve any visionary praxis. He tentatively proposes that *merkavah* speculation was originally "the public exegeses of Ezekiel's vision, which, I presume, accompanied the recitation of Ezekiel 1 in the synagogue on Shabuot" (p. 182), and the danger lay in erroneous private or public exegeses of Ezekiel. Later rabbinic redactors ascribed to earlier rabbinic figures like Rabban Johanan ben Zakkai and his disciples a preoccupation with secret mystical visions. They did so, Halperin argues, to bolster their own religious authority. If the early rabbinic masters were bearers of secret knowledge about the Deity, as was Moses, the later rabbis could claim to be the legitimate heirs of both.

Although Halperin promises a fully developed

study of his new model of the original "real" *merkavah* activity, his critique of Scholem's interpretation would be more balanced had he developed his alternative model here. Moreover, although he may be justified in ignoring apocalyptic sources when investigating the later rabbinic writings, he is less entitled to put aside the *hekhalot* texts if only because Scholem claimed that they were roughly contemporary with the rabbinic sources. Form-critical analysis might have tested Scholem's dating and his assumptions about the relevance of *hekhalot* sources for a proper appreciation of the rabbinic ones.

These limitations of Halperin's study aside, his book is a major achievement of incisive, if sometimes excessive, analysis and argumentation. Gruenwald's survey offers learned and stimulating associations for further research, but Halperin's also shows how we must get out of the shadows of great scholars and try instead to sit on their shoulders so that we can all see further.

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PIERRE DOCKÈS. *Medieval Slavery and Liberation*. Translated by ARTHUR GOLDHAMMER. Chicago: University of Chicago Press. 1982. Pp. vii, 291. \$27.00.

Almost fifty years ago, James Westfall Thompson, the well-known medievalist who was associated with the University of Chicago for most of his career, wrote a book on the dissolution of the Carolingian fisc. In a review of the book, Marc Bloch concluded that anything could be published in the United States. It is therefore somewhat ironical that the University of Chicago Press has now published a book inspired, so the author states, by a question Bloch posed shortly before his death in 1944 at the hands of the Gestapo. The question was: "How and why did ancient slavery come to an end?" It is a legitimate question that this book in no way answers. What a shame that when resources for the publication of scholarly works are presently so meager the University of Chicago Press should waste any to translate and print a book which, to paraphrase Bloch, suggests that anything with the *Annales* imprimatur can be published today in France. To reproduce such a book is a disservice to Bloch and Lucien Febvre because its methodology and use of evidence bear absolutely no relation to the aims toward which these founders of *Annales* history strove in the two decades prior to World War II. Both, despite their revolt against traditional political, diplomatic, and biographical history, respected facts derived from research on primary sources in archives and libraries.

By contrast the closest Pierre Dockès comes to

using primary evidence is a lengthy paraphrase of Columella's *De re rustica*. His principal sources are Marx, Engels, Stalin, and Mao plus a select group of historians who have studied ancient and early medieval slavery and who are convinced that the history of mankind is that of the exploitation of the weak and inferior by the powerful. Moreover, by painting on a canvas spacious enough to encompass slavery in the Antilles and the American South, a slavery seen only through the writings of such as Robert Fogel, Stanley Engerman, and Eugene Genovese, Dockès creates a special patina. Why is it that C. Vann Woodward, Kenneth Stampp, and other scholars of antebellum slavery receive no mention? Could it be that ideologically they are not attuned to Dockès?

The long introduction of forty-six pages defining slavery and outlining methodology, a fifth of the book, presages what is to come. To approach and study a historical problem without possessing an ideology, model, or theory is to Dockès stupid. Apparently, the search for facts and an attempt to interpret or explain them is a pointless exercise. No wonder Dockès is contemptuous of facts; no wonder he dips into Charles Verlinden's fact-laden volumes on medieval slavery grudgingly and only when ideology does not enable him to skip on to his next model-based argument. Although we read on page 112 that "we are going to be looking at the work of historians whose profoundly new approach to the interpretation of history has excited our interest, the historians of the *école des Annales*. . . . Nevertheless, we shall not on that account neglect the work of so important a specialist as Charles Verlinden, whose views we shall have occasion to criticize; what is most interesting in his work, however, is its factual content, 'theory' playing a quite limited role," we question why Dockès pays even this slight courtesy to facts when he tells us that "we shall try to preserve what seems to us Mao's key idea: the decisive, leading role of the relations of production, and hence of the class struggle" (p. 46).

But what about the decline of slavery? Despite the book's title, it deals mostly with the erosion of slavery in the late Roman imperial period. Why did the *latifundia* slave system wither away? Because, states Dockès, the political authority of the state that sustained the system crumbled in the third and fourth centuries, enabling the class struggle (widespread slave revolts) to put the masters to rout or to force them to develop the manorial system. The sixth, seventh, and eighth centuries may well have been a kind of golden age for those who did the dirty work in the early Middle Ages because the lack of political authority enabled the exploited to gain economic, social, and legal liberties. How interesting that Dockès is so sympathetic to the idea of collective Germanic freedom in this period, something that

both Marx and Engels touted, not to mention the discredited *Markgenossenschaft* historians. Then came the talented Carolingians with the attendant resurgence of strong political authority. Obviously, this brought the return of much slavery. But where is the evidence? "As a working hypothesis, Bonnassié suggests that the total number of slaves amounted to somewhere between 10 and 15% of the number of free men. Why not! The figure seems quite reasonable, but the fact is that no one has the slightest idea" (p. 93). If Dockès is so devoted to *Annales* history, why does he not dirty his hands by counting the slaves listed in the Carolingian *polyptyques*? Fortunately, slavery dissolved along with the Carolingian edifice in the later ninth century and again, *ca.* 900, came another relatively free and happy time for the oppressed agrarian proletariat. Then arose feudalism and what Dockès labels "encastlement," when feudal lords planted castles amidst their estates so as to control their peasants, no longer slaves, but still socially, economically, and legally dependent individuals.

This book is a strange revision of the history of slavery's decline during the late imperial period and the early Middle Ages. That it is based largely and unfortunately upon ideology and models precludes serious scholars of the subject from learning anything. They know from what evidence is available that the class struggle, that is, large-scale slave revolts, does not account for the liberation of slaves in the period between 300 and 1000. Even Dockès admits: "Of these silent struggles, especially in antiquity, we know nothing" (p. 208). Though not recommended for historians, this book may appeal to political scientists and sociologists who will be intrigued by the social scientist vocabulary and the models (see especially pp. 186–96). Even Bodin's *Les six livres de la République* is used as evidence of slave struggles in the early Middle Ages! This is an astounding book. It shows "that social struggles explain the end of the slave system (*Gut-slavenwirtschaft*)" (p. 113); it admits that the source material—"the raw stuff of history—is sorely lacking in this regard" (p. 114), and that "one is forced to do a good deal of hypothetical speculation" (p. 179).

Enough! Would that Marc Bloch could review this book! The translation is remarkably good.

BRYCE LYON
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ANNE REIBER DEWINDT and EDWIN BREZETTE DEWINDT. *Royal Justice and the Medieval English Countryside: The Huntingdonshire Eyre of 1286, the Ramsey Abbey Banlieu Court of 1287, and the Assizes of 1287–88*. In two parts. (Studies and Texts, number 57.) Toronto: Pontifical Institute of Mediaeval Studies. 1981. Pp. xiv, 523; 526–766. \$49.00.

The medieval English countryside of which this work tells is the county of Huntingdonshire in 1286–88. In surviving records from those years, royal justice appeared in the county with a general eyre, a special session for Ramsey Abbey, and three sessions of justices of assize. Anne Reiber DeWindt and Edwin Brezette DeWindt edit all these records in the original Latin. In the introduction they assess the place of royal judicature in the lives of the people, concluding that it was important inasmuch as it regulated a share of the everyday concerns of most men and women.

The records are edited with exemplary thoroughness. Nothing is omitted, and generous cross references and good indexes make everything easily accessible. From notes of amercement in individual cases, for example, the reader is referred at once to the entries in the estreats that show the amount of each penalty. But the editors have gone beyond editing. They add a remarkable "Biographical Register" that digests the information that can be found in other sources for any of the nearly three thousand individuals named in the records. From the records and the register the editors can classify more than half of the individuals named in the records in broadly defined social groups. Their classification is convincing. It allows them to demonstrate that, as far as can be known, 71 percent of the named individuals were simple villagers and that such people made up 53 percent of plaintiffs and of defendants in civil cases. The social analysis continues in detail, considering the types of cases that involved each social group, the outcomes of litigation between members of different groups, and much else. It is a brilliant study.

Amid so much, there must be something to cavil at. The editors often read *u* (= *v*) as *n*, so that *Lefeuere*, Smith, becomes *Lefenere*, "Fenner." Latinists will wince at the repeated "Nicholus" for "Nicholaus" and "Matillis" for "Matillidis" or "Matillde." Other niceties of editing sometimes go awry. What is more important, users of the introduction should note that the editors generate some statistics upon principles that may not be right for other studies and that occasionally seem wrong. For example, among civil actions in the eyre the editors count 42 actions of Entry and 29 of Right. By other lights I counted 36 of Entry and 17 of Right. The editors count 35 felonies that were prosecuted by "appeal," 14 by indictment, and 235 by presentment. I found 16 by appeal, 10 by indictment, 12 upon suspicion, and only 16 by presentment. Though arraignments could be held upon them, presentments were not intended to prosecute felonies but to review the policing of the county; their true subject matter was not the felonies that they mentioned but failures to arrest, abjurations, escapes, and the like. Even in telling of felonies they

told routinely only of homicides and of others only in special circumstances. The types of felonies shown in the records (p. 51) should not, therefore, be taken (p. 63) to represent the proportions of different kinds of felonies that were committed.

DONALD W. SUTHERLAND
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ROBERT C. PALMER. *The County Courts of Medieval England, 1150–1350*. Princeton: Princeton University Press. 1982. Pp. xvii, 360. \$35.00.

This is the first in-depth study of the medieval English county courts since W. A. Morris's *The Early English County Court* (1926). It provides detailed description of the range of these courts' activities and their place in the emerging legal system at the key period in their existence (1150–1350), along with new analysis of the dominating elements within them, which Robert C. Palmer argues were the legal experts of the magnates rather than the amateur knights of the shire long supposed by historians to have been the influential group.

The book is divided into two parts, entitled respectively "Institutional Framework and Personnel" and "Jurisdiction." What the author describes as the third section or companion volume, a volume of edited sources, is to be published later by the Selden Society. Anticipating a time-lag between these publications, the author has tried to reproduce sufficient evidence in the notes, but the advance references to a volume that, I am told on inquiry, is unlikely to appear before 1986 or 1987 remain tantalizing.

The first two chapters, "Venue and Scheduling" and "The Sheriff and His Staff," form the section for nonspecialists and will appeal to readers interested in local and administrative history but not themselves legal experts. (Indeed, the second chapter could be recommended on its own as a guide to county administration in this period.) The remaining chapters in the first part—"Suit and Judges," "Professional Lawyers," and "Seneschals and Bailiffs"—argue that the courts were dominated by legally expert baronial seneschals and bailiffs even before the reign of Edward I, that professional lawyers made the county court professionally and legally respectable, and that the evidenced twelfth-century activities of seneschals and bailiffs make them "the first secular lawyers in England and . . . the lower courts . . . more important than the king's court in the genesis of the profession" (p. 136).

The second part is more technical. In the chapters "Record, Removal, and Supervision" and "The Viscontiel Writs," attention is turned to the processes developed in the twelfth and thirteenth centuries that integrated the various English courts into a legal system with standardized writs and truly com-

mon law. The chapter "Personal Actions: The Reign of Edward I" concentrates on developments in a reign when the judicial importance of the county was reduced. The penultimate chapter, "County, Courts, and Country," looks at the thirteenth-century county courts' relationship with other courts (especially those of liberties and ancient demesne and Parliament) and includes an interesting survey of the effects of eyre visitations. In the conclusion Palmer underlines the theme of curial integration, achieved in the thirteenth century and subsequently lost.

A very impressive study of original sources underlies this work, which is compiled in a stimulating and provocative way. Seven appendixes, comprising tables (of county days, venues, pleadable writs, and fine roll statistics), Latin texts of documents translated or summarized in the text, and a note "Crown Pleas," are provided, with an excellent glossary of legal terms and Latin words, a selective bibliography, and an index.

HELEN M. JEWELL
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ROBERT S. GOTTFRIED. *Bury St. Edmunds and the Urban Crisis, 1290–1539*. Princeton: Princeton University Press. 1982. Pp. xvi, 313. \$27.50.

In recent years something of a debate has arisen among British scholars as to the economic state of English towns in the later Middle Ages, and it is therefore all the more welcome an event that a transatlantic scholar has now elected to cast a cool eye over the fray. Robert S. Gottfried, whose work on epidemics during the same period has already aroused a good deal of curiosity, here turns to a more detailed study of one remarkably interesting town during these controversial years. He does so in eloquent brush strokes on a wider chronological canvas than is nowadays usual. In this new book he sets an example that the rest of us disputants might emulate: he looks in some depth at the history of his chosen town—the abbey-dominated borough of Bury St. Edmunds in Suffolk—over nearly two and a half centuries, from 1290 to 1539. He pursues two major themes. On the one hand, he traces the gradual and frequently stormy emancipation of the burgesses from the domination of the abbey and its powerful franchises. On the other hand, he seeks to demonstrate that, despite the generally prevailing demographic difficulties of the time, the flowering economy of the town, together with its prosperous cloth-producing hinterland, was not only proof against what your reviewer has tentatively postulated elsewhere as an age of "urban crisis" (hence that part of Gottfried's title) but was also the necessary

precondition for the burghal response to the anachronistic dead weight of the feudal tradition.

In a sense, then, the author has sought to write two books. The theme of the former is already broadly familiar through the earlier work of M. D. Lobel, whose findings are constantly quoted and occasionally adjusted. The second theme arises initially from the author's previous preoccupation with the rich probate material of the area, but this interest seems to have been linked with the general debate on urban fortunes only rather late in his thinking about the book. Both the brief introductory survey of the wider controversy and the short conclusion on the wider significance of the study look as though they have been grafted on to what was originally a local investigation. The general debate, moreover, pursues the problems of England's towns down to 1570, whereas this book ends with the dissolution of the monastery.

At the heart of Gottfried's view of Bury in this wider context lie two claims about what differentiated the town from those others that were declining: its demographic resilience and its regional economic significance. In the former instance he proposes a series of "guesstimates" for the total population: 7,150 in 1347; 4,200 in 1377; and 5,438 in 1522. He concludes (p. 247) that "by 1540 Bury, unlike virtually all other important provincial centers, probably had as many people living in it as it did before the plagues." This is a bold statement indeed, when on much the same evidential bases, alternative guesses might be made to suggest either that the population totals may have been much the same in both 1377 and 1522—say 4,890—or that the total may actually have dropped by the latter year. John Patten has suggested the very low figure of 3,550 in 1522 from the same source, and it is noticeable that—*pace* Gottfried—the evidence for "testamentary mortality" between 1441 and 1530, reveals the worst decades as 1471–80, 1501–10, and, most serious of all, 1521–30. As the author is at pains to point out, but not to enumerate, a major reason for the abbey's financial weakness on the eve of dissolution was its inability (as the holder of practically all the property in the borough and its suburbs) to find tenants. It is certainly possible that some townsfolk were buying cheap leases and that the outskirts of the town were being newly developed, but in the author's words (p. 43) "the inner city began to rot" and "dozens of properties" were vacant. If this demographic evidence is, to say the least, ambiguous, so too is the weight that the author has to place on the dubious testimony of male-replacement rates calculated from wills. In short, his demographic interpretation is not proven, even though it is not difficult to agree that Bury's experiences were less drastic than those of other towns.

That this was due to the borough's situation close

to the wealthy rural cloth-producing areas of southern Suffolk and adjacent Essex is not to be doubted. The author brings out well the familiar picture of mercantile connections with the surrounding countryside and suggests strong links with nearby Lavenham. Would that he had been able to pursue these matters into the post-dissolution period. To have done so might have revealed the degree to which Bury's relative importance was equally due to the economic significance of the abbey itself.

CHARLES PHYTHIAN-ADAMS
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ROBERT A. DODGSHON. *Land and Society in Early Scotland*. New York: Clarendon Press of Oxford University Press. 1981. Pp. xii, 345. \$54.00.

In recent years Robert A. Dodgshon has made a major contribution to the study of the evolution of field systems in Europe with his emphasis on the importance of tenurial conditions and landholding structures. His theories on the origins and development of field systems are, however, based on detailed research on medieval and early modern Scotland. His work on Scotland has appeared in a number of articles in historical and geographical journals, but in this volume it has been brought together and expanded along with much new material. This is the most important book on the evolution of agriculture, settlement, and landholding in Scotland to have appeared for many years and, on account of its wide scope and distinctive approach, it is likely to generate much controversy and, one hopes, research in years to come.

Research and writing on the evolution of agrarian structures in Scotland has, of necessity, gone through a phase of superficial generalization, exemplified by the work of T. B. Franklin and J. A. Symon in the 1950s. More recently it has progressed through an intensely empirical stage of detailed source evaluation, made particularly necessary by a persistent, if exaggerated, belief in the shortcomings of Scottish documentation and an overreliance in the past on the forcefully propagandist writings of eighteenth- and nineteenth-century agricultural improvers. If scholarship in this field is to attain real maturity, it now has to move forward and adopt a more theoretically based approach. Dodgshon successfully makes this transition, though of necessity he is still concerned at times with demonstrating the potential of neglected sources such as the *Register of the Great Seal*, whose use in settlement studies he has pioneered. As a result, some Scottish historians, who have traditionally worked with a focus that has been narrow sometimes to the point of parochialism, may find his approach disconcertingly broad and wide-

ranging over both time and space. Historians generally will, however, be able to relate trends in Scotland more firmly to those in other areas. The advantage of Scotland for research of this kind is that many social and economic structures—such as agricultural rents, field systems, and farming practices—persisted into the seventeenth and eighteenth centuries, long after they had disappeared in more developed areas. They thus survived into periods that are comparatively well documented, allowing them to be analyzed in depth and then related to patterns in more remote time contexts.

The author reviews the relationships between land and society from prehistoric times until the eve of the most rapid phase of agricultural improvement in the later eighteenth century. Most of the volume is concerned with the detailed operation of the farming township, along with broader aspects of rural society and rural economy from medieval times onwards, but there is also a challenging chapter on patterns of territorial organization in earlier Scottish society. This is a densely packed book, one to be read slowly and thoughtfully, but handsomely repaying the reader's attention.

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BERNARD F. REILLY. *The Kingdom of León-Castilla under Queen Urraca, 1109–1126*. Princeton: Princeton University Press. 1982. Pp. xx, 401. \$32.50.

Because the royal archive of medieval Castile was destroyed, scholars have to tackle individual reigns by collecting the monarch's surviving charters from minor archives, constructing itineraries, and collating them with narrative sources. Fewer than half the monarchs have so far received this treatment, but now Bernard F. Reilly has provided it for Urraca of León, almost the only queen regnant in the medieval West. Apart from two local chronicles, his main sources were over a thousand documents, including 118 royal charters that, after sharp diplomatic and paleographic analysis, he approves as authentic.

In the first half of his book, he presents the first clear and well-documented narrative of Urraca's political career, that is, her continual quarrels, alliances, battles, and truces with her husband Alfonso I of Aragón, her half-sister Teresa of Portugal, and Bishop Diego Gelmírez of Santiago; it is a narrative that is unlikely to be superseded or significantly altered by further research. Reilly concludes that his heroine usually made the right decisions and was ultimately successful in handing on her dominions, relatively united and at peace, to her son; and he stresses that all her rivals aimed to control the whole of Urraca's dominions, and that the losers settled for

Portuguese and Aragonese independence only as second-best—a view unlikely to please nationalist historians in either area!

In the second half of the book, Reilly surveys the major institutions: court, episcopate, taxation, royal administration, and towns. Other historians have already studied each of these separately over centuries-long periods, but this is the first attempt to study them together, intensively, over a short period and from strictly contemporary documents. It leads to the conclusion that most institutions were still in a very early stage of development and that the autonomous town councils, for example, as yet hardly existed. In all this, Reilly is admirably knowledgeable, thorough, and critical, and his challenges to orthodox views will have to be taken very seriously.

Like every good author, he provokes questions and demands for more. One wonders, for example, whether he might not have dug more out of Muslim chronicles (for example, about Almohade pressures on the different Christian factions, or about the battle of Valtierra), or out of Aragonese, Portuguese, or French sources. And one would have welcomed an explanation of the criteria for determining whether persons mentioned in a charter were present at its issue. For example, does the charter of 1118 (p. 136) really prove the presence of Alfonso Raimúndez, Bernardo of Toledo, and Pedro of Palencia? Was that of 1110 (p. 69) confirmed by the abbot of Silos or of Husillos? And did Urraca really not help to grant that of 1124 (p. 188)? Lastly, surely *Vallibriense* is not a place-name but dog-Latin for “pertaining to Villamayor de Brea.”

But these are very minor points. And though one might wish for a genealogical table to accompany the excellent maps, and even for an edition of Urraca's charters, one must be realistic. In reality, this is a marvelous book, the fruit of many years of intellectual and physical labor; and it contains not only vast amounts of information about documents, persons, and events, but also valuable insights into one of the darker periods of Spanish history. Reilly has not only performed a “work of giants” in the hard grind of collection, analysis, and synthesis; he has also shown us the reality of twelfth-century Iberia, uncluttered by hindsight, modern ideologies, or presuppositions about the ways in which the peninsular kingdoms allegedly had to develop. His book is a model of its kind and will remain a classic until well on into the next century.

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WERNER MORITZ. *Die bürgerlichen Fürsorgeanstalten der Reichsstadt Frankfurt a.M. im späten Mittelalter.*

(Studien zur Frankfurter Geschichte, number 14.)
Frankfurt a./M.: Waldemar Kramer. 1981. Pp. 279.

This monograph is a detailed study of civic welfare institutions in Frankfurt during the late medieval period. Originally a dissertation written under Peter Herde at the Goethe University, the book deals with a cluster of charitable foundations from their beginnings in the thirteenth century to their reorganization into a more unified alms agency, the *Almosenkasten*, in 1531. Werner Moritz has made it his task to re-examine all the evidence still extant after the heavy losses to the Frankfurt City Archive in the Second World War, a worthwhile but frustrating enterprise because of many gaps in the documentation and the fact that the catalogues of archival holdings have not even been properly updated over the past thirty-five years. Moritz inspires confidence that he has seen everything available, and he has provided an absolutely reliable collection of information useful to Frankfurt researchers as well as students of medieval welfare agencies.

Although the previous book in this series of “Studies in Frankfurt History” was much too theoretical (see my review, *AHR*, 87 [1982]: 475–76), Moritz has leaned too far in the opposite direction. He presents a satisfyingly concrete description of institutional arrangements—physical location, increasing civic control over private foundations or bequests, administrative organization and practices, and the care of both wealthy retirees and the poor—as well as a description of the economic resources that supported the community's welfare agencies. But the author remains descriptive and preoccupied with getting his facts straight; important as that job is, it is no substitute for historical analysis. Here Moritz seems to have defined his research in terms of confirming or slightly modifying the generalizations of Siegfried Reicke's major two-volume institutional study, *Das deutsche Spital und sein Recht im Mittelalter* (1932). Certainly he has not offered us a dynamic contextual analysis that would situate public welfare among other social or economic developments in Frankfurt over the late medieval period. Moritz's picture is too static and has by no means displaced the old accounts by Georg Ludwig Krieg or Friedrich Bothe. His discussion of the economic resources supporting the charities tends, unfortunately, to become a listing of properties or bequests they acquired. Moritz's treatment of various poor-relief agencies clarifies legal relationships and administrative organization but offers no real insight into the demands for change in 1525 or the reorganization of civic welfare in 1531. Thus his book will prove useful to specialists who want reliable information on its subject and will disappoint readers

who expect a historian to go beyond collection of facts to the meaningful interpretation of data.

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NORMAN HOUSLEY. *The Italian Crusades: The Papal-Angevin Alliance and the Crusades against Christian Lay Powers, 1254–1343*. New York: Clarendon Press of Oxford University Press. 1982. Pp. 293. \$44.00.

In this study of political crusades in Italy, Norman Housley challenges the dominant view that regards them as "the degradation of the crusade" or, at the very least, as diversions of men and resources from the Holy Land. Instead, he argues that they were true to the ethos of the crusade movement and that they garnered wide financial support and a broad popular following. Waged in defense of ecclesiastical liberties and the temporal domain of the papacy in Italy, they supported and drew support from the *parte Guelfa* against the Ghibelline enemies of the church. Housley carries his story from the struggles against Manfred to the invasion of Italy by Louis IV of Bavaria. Much of the book deals with the trials and tribulations of the Angevins, including support for Charles I's invasion of the *Regno* and the War of the Sicilian Vespers. But it is not merely a retelling of these events in terms of the crusades preached to support them. Housley examines the manner in which the papacy and its supporters justified the use of the crusade, pointing to evidence of restraint both in the proclamation of crusades and in the delineation of areas where they were to be preached and from which taxes were to be raised for their support. He finds support for the view of the papal curia that these crusades were necessary preambles to the successful launching of campaigns in the Holy Land. He responds to the argument that they represented a diversion of resources from the East. His lengthy chapters on the financing of the Italian crusades make a valuable contribution to the history of papal fiscal management and policies in the thirteenth and fourteenth centuries. There is no question that from these pages there emerges a more complex and nuanced picture of papal use of the political crusade than that found in most of the literature. In fact, it is as an explication of positions taken by the papacy and its allies that this work makes its contribution.

Unfortunately, Housley was not content to rest his case at that point. By attempting to reverse present views instead of offering a balanced critique of their weaknesses, he has, in my view, far exceeded his evidence. The problem may be illustrated by the fact that he not only minimizes criticisms of papal crusade policy, but also attempts to isolate those dealing with papal taxation from those direct-

ed against the political crusades. Although there is some justification for making this distinction as part of the analytical process, it seems to go too far to separate these two types of criticism in terms of their net effect on public opinion. Much of Housley's argument rests on a view of public opinion that seems rather ill defined. His discussion of the popular response to preaching depends too much on reaching the same conclusions that he does regarding the interpretation of rather vague references to the numbers of crusaders and the nature and motivation of the mercenary forces. His view of the *parte Guelfa*, stressing its religious ties, needs further development before it can claim to oust from the field the interpretation that casts the Guelfs in a strongly political light. At times the narrowness of his vision leads him to pass over too lightly a problem that seems to run counter to his thesis. Such is the case with his references (pp. 180 and 201) to King Henry III of England, whose problems with the barons were certainly exacerbated by his involvement with the papacy in the Sicilian affair.

Yet, despite these criticisms, this study does show that not only was there a case to be made in support of the employment of the crusade against the enemies of the papacy in Italy, but also that certain aspects of that case deserve more serious attention from historians. If nothing else, it has added to our understanding of the essentially defensive and conservative posture assumed by the papacy in the later Middle Ages.

JAMES M. POWELL
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THOMAS KUEHN. *Emancipation in Late Medieval Florence*. New Brunswick: Rutgers University Press. 1982. Pp. xii, 247. \$28.00.

In the past fifteen or twenty years Florentine family history has become something of a growth industry. So extensive is archival evidence, particularly in the form of notarial cartularies, that there is little danger of the well running dry. Disgust with the public world and political history in our times augurs well for the busy enterprise of collecting materials pertaining to the private and domestic life of humankind. Unfortunately, few cartularies (only about twenty or so) survive for the thirteenth century, and before this time only scattered documentation exists. The Florentine archives cannot compete with those of Genoa and Lucca when it comes to documentary sources for early medieval history. Not surprisingly, then, investigators of Florentine social history have concentrated on the fourteenth and fifteenth centuries. Because the *catasto* was drawn up in 1427, listing much of the wealth of the city's ten thousand or so households, along with an abundance of demographic data, the lure of the fifteenth

century as a field for social research has proved irresistible. Much of this scholarship is anchored in the research of David Herlihy and Christiane Klapisch-Zuber's *Les Toscans et leurs familles* (1978), complemented by other recent monographs, which place the history of selected families in the broader context of Florentine economic and cultural life.

The present study by Thomas Kuehn utilizes much of the Herlihy and Klapisch-Zuber data, but its concerns are principally legal. The subject of the work, emancipation, is crucial for an understanding of family life and the history of childhood. Therefore, it is necessary to understand the juridical aspects of liberation from paternal authority. A declaration was made before a court setting the process in motion; often, the father endowed the emancipated child with patrimony. Permitted to enter binding contracts and execute transactions, both private and public, such as the making of a will, the *emancipatus* now achieved the status of a legal person. The father and son were released from certain mutual liabilities, and this was a compelling motive for emancipation in a city where merchant credit was the life force. Generally, sons were accorded their majority upon entry into a professional or business career, whereas unwed daughters were emancipated at the death of the mother. The daughter then turned over her maternal dowry to the father. Widows were also emancipated in order to manage the family's property more effectively. Emancipation was often a legal formality, since sons might well remain under the economic control and supervision of the father. Not until he married was the son likely to achieve independence—though the word has too modern a ring. Since well-to-do Florentine males married late, paternal control might well be prolonged.

The problem confronting the government involved prevention of emancipation as a device for defrauding creditors: liability laws were complex and subject to conflicting interpretation. Kuehn suggests that there were roughly two stages in the utilization of emancipation: the first, dating from the mid-fourteenth century, saw the practice directed toward granting majority to offspring at an early age, so that they might be free of joint responsibility in cases of debts. The second stage, dating from the 1420s, saw emancipation used as a strategy for familial and patrimonial adjustment. The state took over registration from the Court Merchant, and the fee charged for making the declaration rose substantially. The number of those emancipated decreased and the age of majority advanced.

No simple explanation for this shift is possible, but it is most likely that the threat of joint liability had diminished by the early fifteenth century. Florentine statutes were strict on this point, but the direction of legal opinion was clearly away from any

literal construction of these Draconian laws. These provisions were regarded by the best jurists in the city as "odious and exorbitant," since they permitted punishment of sons for derelictions of fathers. Kuehn observes that these lawyers were not ideologues, spokesmen for a class, or conservators of the social order. A reading of their opinions proves that indeed they were not servants of power but men of broad social concerns who were repelled by harsh and unyielding legislation on the subject of mutual liability. That they sought to intervene on moral grounds is a matter of cardinal importance for any general interpretation of legal culture as well as family history. The author, however, does not take this evidence into sufficient consideration when drawing conclusions; rather, he drifts off into a bland discussion of the imperatives of honor, patrimony, and family for understanding the manipulative nature of emancipation. Honor, patrimony, and kinship are just as appropriate for interpreting family life in a Mexican village or ancient Rome as they are for patrician-dominated Quattrocento Florence. What is striking about fifteenth-century Florence is the large number of urban children who were members of affluent families. Florentines overcame the limits on fertility which a city normally imposes. According to the *catasto* of 1427, better than forty-one percent of Florentine children belonged to well-to-do families. This datum merits careful consideration when discussing changing patterns of emancipation. Theses pertaining to the distribution of wealth should in this instance underscore the large number of advantaged children in the city. This remains, however, a valuable juridical study of emancipation, even if one dissents from certain of its conclusions.

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ALEXANDER KAZHDAN and GILES CONSTABLE. *People and Power in Byzantium: An Introduction to Modern Byzantine Studies*. Washington, D.C.: Dumbarton Oaks, Center for Byzantine Studies. 1982. Pp. xxi, 218. \$12.50.

Following a preliminary statement to the effect that Byzantine studies are considered to have begun in the seventeenth century, Alexander Kazhdan and Giles Constable begin their book with an examination of the various aspects of this question by discussing briefly the ideas put forth by different scholars, particularly those of H. G. Beck, P. Lemerle, G. Ostrogorsky, G. Weiss, M. Ja. Sjuzjumov.

The question of continuity and change constitutes the principal subject of discussion in the introduc-

tory chapter. The authors suggest that the question is complex and admits of no simple answer. Kazhdan's view on the subject will come out in the course of the discussion that constitutes the main body of the work.

The eight main chapters of the book were originally offered as lectures but lightly revised for the purpose of publication. What one may understand by a "Byzantine" is the subject of the first chapter. A "Byzantine" was a complex individual whose features are not easy to describe. He was the product of developments involving factors that differed at different times. He was one thing today and another tomorrow. The underlying principle of this position is the concept of the relativity of values.

In chapter 2 the accent is put on social and economic relationships. Described in some detail are the economic and social effects resulting from the geographical features of the empire: valleys, mountains, deserts, and so forth. In the chapter that follows, a contrast is drawn between the Christian and classical traditions, and the point is made that the former did not completely eliminate the latter. Included in the account is a description of beliefs and practices, some of which still exist in modern Greece, where the Byzantine church as it had traditionally evolved has, to a point, survived. These theological beliefs and practices are treated somewhat extensively in the fourth chapter.

The last four chapters deal with culture in the restricted sense of that term. Literature, treated in the first of these chapters, is viewed as an expression of human activity. It was not just a compilation of written expressions, generally regarded as dead, but, in a sense, a living organism. The second of these chapters analyzes the position of the individual in society, and the third discusses the extent and quality of the sources. The underlying principle of these chapters, indeed of the whole book, is, to repeat what has already been said, the relativity of values.

Kazhdan is a distinguished scholar known for his various publications. His book is an important contribution to the historical literature on the Byzantine empire. Both in the facts and the interpretations that it offers, it is virtually free of errors. There are, however, some omissions. This reviewer, for instance, failed to notice any reference to his own works on the social and economic structure of the empire, subjects that are examined by Kazhdan somewhat in detail.

The inclusion of Giles Constable in the title of the book as coauthor is explained by two things: (1) Constable's close reading of Kazhdan's manuscript and (2) the additions made by him, referring specifically to Western Europe.

PETER CHARANIS
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HENRY MAGUIRE. *Art and Eloquence in Byzantium*. Princeton: Princeton University Press. 1981. Pp. xxii, 148. \$32.50.

This is an interdisciplinary treatise in which Henry Maguire of the University of Illinois at Urbana-Champaign undertakes to show the strong influence that certain types of literature exercised on the visual arts. Eloquence especially exercised a potent influence on iconography. The impact of the devices of description (*ekphrasis*), antithesis, and hyperbole is studied with care. Scenes where the influence is quite noticeable are the Annunciation, the Nativity, the Presentation, Christ's Passion, the Dormition of the Virgin, and the Massacre of the Innocents.

Maguire had at his disposal a substantial amount of evidence to support his thesis successfully and convincingly. He points out that the classical models of description or *ekphrasis* by the pagan orator Libanius exercised a strong influence on the Christian preachers whose sermons and letters had a strong influence on the visual arts. Maguire undertakes to show in particular how the techniques of rhetoric passed from the schoolroom into the literature of the church and from the literature of the church onto its walls. The period covered is 843–1453.

The greatest task of Maguire in this work is to identify a number of nonbiblical texts that influenced the structure and imagery of Byzantine art. Maguire reaches the conclusion that Byzantine rhetoric influenced Byzantine art in three main ways. It helped artists to add vividness to their art with dramatic detail. Rhetoric helped artists to structure their compositions, especially antithetical parallel scenes. Lastly, rhetoric helped artists to enrich their works with images.

The work is provided with over one hundred illustrations. As a fine interdisciplinary treatise it helps in establishing the close relationship between art and eloquence in Byzantium.

JOHN P. CAVARNOS
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MODERN EUROPE

JOHN R. LOESCHEN. *The Divine Community: Trinity, Church, and Ethics in Reformation Theologies*. Kirksville: Sixteenth Century Journal, Northeast Missouri State University. 1981. Pp. 238. \$15.00.

This book by John R. Loeschen is a careful study of the associations between the trinitarian theologies and social theories of three Protestant reformers, Martin Luther, Menno Simons, and John Calvin. Except for Max Weber and those who have struggled over his thesis, there are not many historians

who have tried to march directly from the reformers' theologies to their social theories. Loeschén acknowledges this briefly and then proceeds to examine the way doctrines of the church and theories of society are rooted in the trinitarian theologies of the three church leaders. He is not interested in formal agreement with Nicaea—for all were orthodox—but in the differing ways the three persons of the Trinity function for each. The progression in analysis moves from Trinity to church and then to social theory.

To give an example: Loeschén finds Simons's radical rejection of the sinful world rooted in his doctrine of the Holy Spirit's role as founder and purifier of the church, whereas Calvin's equally strong desire to reform society is rooted in the function of the Father as creator of all *and* as founder of the church. The Spirit's work is particularistic, concerned only with the pure communion of saints, hence Anabaptist separatism. But the Father's work is universalistic, concerned with the whole of humankind, hence Calvinist reformist social theory. Theology precedes sociology.

Several questions arise for the historian: Is this a proper way to do history? If it is, has Loeschén chosen the best place to examine the theologies behind the social theories? If so, has he done it well?

To the first, there is of course no adequate answer. Historiography has no reigning orthodoxy, and Loeschén is aware that his method can be called naive. But he proceeds as confidently and singlemindedly as a Marxist, and that is unsettling.

To the second, I think that a thorough examination of the functions of, and relationships between, the divine and human natures in Jesus Christ might prove more rewarding. For there we see in a nutshell how God relates to man and how God's spokesmen (Luther, Calvin *et al.*) relate to humans in society.

To the third question—has he done it well?—I answer affirmatively, but with the qualification that the reader tends to lose the main outline as the theological exegesis goes on and on. Moreover, in the case of Menno's *Foundation Book* there is a statistical study of nouns that refer to the Trinity. Although this is probably a new use of quantitative measurement in writing history, it is a very confusing study, and the reviewer was not helped by the tables. Loeschén studies each reformer in turn and relates the three at the end of each section. The last chapter surveys the decrees of the Council of Trent in order to draw comparisons with the three Protestants, and that is very helpful and the clearest section of the book.

Had the author only worked in the area of social theory, one could argue that he discovered in the three trinitarian conceptions what he needed to explain already well-known social theories. But a

number of side issues of interest to Reformation scholars are also clarified by Loeschén's taxonomy, and this testifies to the general validity of his approach. Luther on ubiquity, Calvin's *extra calvinisticum*, and Menno's celestial flesh of Christ—all these doctrinal stands are rooted in trinitarian, not in eucharistic doctrine.

Two forthcoming works on Calvin by William Bouwsma and Suzanne Selinger will be striking contrasts to Loeschén's method. These literary analyses will seek to discover the source of his work in his psychological makeup rather than in his theology. The stir among students of Calvin will be considerable and will help us evaluate Loeschén's purely theological approach.

W. FRED GRAHAM
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LANDO LANDI. *L'Inghilterra e il pensiero politico di Montesquieu*. (Pubblicazioni della Università di Pavia, Studi nelle Scienze Giuridiche e Sociali, new series, number 32.) Padua: CEDAM. 1981. Pp. xiii, 752. L. 35,000.

For an American reader, the works with which Lando Landi's book most readily invites comparison are *Montesquieu's Philosophy of Liberalism* by Thomas L. Pangle and especially *Montesquieu and the Old Regime* by Mark Hulliung. Like these interpreters, Landi clearly recognizes the centrality of the "English model" in Montesquieu's thought; like them, he avoids a narrow legal or political analysis of Montesquieu's view of England and shows how the author of *The Spirit of the Laws* was concerned with the broad sweep of English history and institutional development. Like them, too, he believes that Montesquieu saw in England a new, unique, and admirable form of society and government, though one born from a medieval, territorial monarchy.

Parting company with Hulliung and Pangle, however, Landi argues—quite persuasively—that there is no reason at all to believe that Montesquieu proposed England as a model for any Continental nation or that he saw it as representing the future of all European states. Instead, Landi maintains, he saw the English model as one way of achieving liberty; but he believed that the corporate institutions of France, far from being things of the past, were quite capable of achieving the same end, albeit on a more moderate scale. Further, more than other scholars who have examined the role of England in Montesquieu's thought, Landi carefully explores eighteenth-century English society and government, showing how Montesquieu perceived (and misperceived) them in creating his model. Landi also calls attention to the role that England played in the development of Montesquieu's typologies of monarchies and republics—a topic insufficiently examined

by earlier students. Thoroughly aware of the evolution of Montesquieu's ideas, Landi demonstrates how a knowledge of England and of English writers helped to change his ideal from ancient, self-sacrificing virtue to modern freedom in the sense of individual security.

With massive erudition—this book would be worth acquiring for its bibliography alone—coupled with a lively, clear, and graceful style, Landi explores the subtle tensions in Montesquieu's works: between “modern” liberalism and “feudal” corporatism; between a keen appreciation of the particular social and geographic conditions of nations and a stress on the universal principles of liberty and moderation; and between synchronic or “functional” perspectives and historical perspectives. In the process of examining Montesquieu's views on England, Landi presents a lucid and compelling interpretation of the philosopher of La Brède. In sum, this is probably the most important book on Montesquieu in recent years, and it is essential reading, not only for students of Montesquieu but also for anyone interested in eighteenth-century social and political thought.

DAVID YOUNG

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F. L. CARSTEN. *War against War: British and German Radical Movements in the First World War*. Berkeley and Los Angeles: University of California Press. 1982. Pp. 285. \$22.95.

War against War is a lively, engaging title. The cover leaf also alluringly boasts that the work “breaks new ground,” mines “new archival sources,” offers “fresh insights,” and is blessed with a “clear, fluent style” that “will attract a wide readership amongst historians and the general public.”

Such a buildup. And such a letdown.

Although this is a work of unquestioned diligence, accuracy, and clarity, it is also one of surprisingly narrow perspectives. It sticks to its sources like the first draft of a Ph.D. dissertation. Furthermore it offers precious little that is new in any rigorous sense to historians familiar with recent scholarship dealing with the socialist left during World War I. For the most part it supplies details that elaborate on familiar accounts or generalizations that have become commonplace. New vistas, efforts to challenge received wisdom, and new interpretive tools almost never appear. As far as the general reader is concerned, he too is not likely to find much, either in freshness of approach or in skillfulness of writing, that will sustain him through the thickets of detailed narrative. No personalities emerge, and even the dramatic events that fill these years are offered up in a flat, colorless way.

F. L. Carsten's claims in the preface are modest,

and it may be unfair to fault him for the hyperbole that found its way to the cover leaf—we all hope to find a wide readership, and even university presses are not entirely immune to the temptations of Madison Avenue. He recognizes that the left in Great Britain and Germany during World War I has been much studied. His goal is simply to supplement the existing literature, since there now exists “no modern study of the Left as a whole in either country and certainly no comparison of the two.” Yet even these claims are shaky and need clearer formulation.

This work does not study the left as a whole, or “Radical movements” (as given in the subtitle), but rather the socialist left (“Radical” as I understand it includes the nonsocialist left). Much more importantly, *War against War* compares German and British socialist only in the most perfunctory way. For the most part their stories are juxtaposed in separate chapters; comparisons are infrequent and unambitious to an almost puzzling degree. Aside from an extremely brief preface, no introductory or concluding remarks are provided to tie the work together—a particularly inexcusable lapse in a book that claims its contribution derives partly from its comparative approach. And finally, the theme of war against war often gets lost in what is simple party history, especially regarding the Social Democratic party.

One may or may not esteem what has been accomplished in the name of quantification in historical studies, but Carsten's volume is an almost perfect example of the kind of traditional narrative to which the cliometricians have objected. To pile up account after account of individual antiwar actions, for example, can lead to a false sense of the pervasiveness or quality of antiwar feeling in the general population. Fifteen examples are no more persuasive than five—nor less so than thirty—when we have no clear sense of proportions or percentages. To write that the German people in the course of the war became ever more bitter or that German sailors' hatred of officers became ever more intense is no doubt true but not very enlightening, especially when such airy adjectives are resorted to repeatedly. To avoid such vagueness is of course often extremely difficult, and most historians find it necessary to provide what are subjectively judged to be typical or fair examples and to use descriptive terms that lack precision. Nevertheless, Carsten seems often oblivious to the problem.

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GORDON BROOK-SHEPHERD. *November 1918*. Boston: Little, Brown. 1981. Pp. 461. \$19.95.

Gordon Brook-Shepherd's most recent book on the Great War treats its ending, the last one hundred days from the "black day" of the German army at Amiens on August 8, 1918, to the silencing of the guns at eleven in the morning on November 11. It is his aim to show that the ending of the war was not a mere denouement or the collapse of a formidable social-military-industrial machine, but a genuine "conclusion" given the logic of force prevailing on the four major battlefields—the Western front, Italy, the Balkans, and Palestine. Here is the classic tale of the unfreezing of the front, the growing string of Allied successes, the slow realization by the leaders of imperial Germany that they had lost, and their frantic attempts to escape the worst consequences of their defeat.

This too is conventional military history written within what John Keegan has called the "narrative tradition," which typically centers upon the "battle piece," reduces soldiers to pawns within the larger framework of strategy, and places a high focus on leadership. Indeed, it is the assumption of narrative history that events are "authored" by those in command, and that one can read the character of leaders in the outcome of events. Thus the impetus of the final Allied offensives on the Western front has something to do with Marshal Foch's characteristic energy, for he is imbued with the "granite and strong light" of his native Pyrenees and suffused with an "inner radiance." Also characteristic of the narrative tradition of military history writing is the treatment of those in combat as instrumentalities of a higher will. Masses of troops can be defined in terms of their tensile strength, their powers of resistance. They exist to be thrown into the line to plug gaps or turn flanks. They are "unleashed" by the generals who direct blows at each other through this human materiel.

However distant from the realities of war, the assumptions characteristic of narrative history produce a dramatic structure that is pleasing to a mind grown impatient with the monumental confusion of events as experienced. Perhaps this is why it continues to be written and why it continues to stand as "History" in the popular mind. And yet these assumptions stand in the way of an understanding of the very "modernity" of an event like the First World War, for this was a war in which millions of men concluded that they were participating in an event wholly separate from them, autonomous, dictated by the logic of industrial "mechanism." If the war had an effect upon European culture in general, it had this effect within those individuals who inhabited the category of "masses." Any historian who treats great and catastrophic events cannot ignore Tolstoy's proposition that these events are affairs of a "people" who often bear a problematical relationship to the founts of authority. The history of

such cannot be written as if it were produced by formal authorities any more than a performance of a symphony can be treated as a product of the conductor's skill with a baton.

ERIC J. LEED

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DAN P. SILVERMAN. *Reconstructing Europe after the Great War*. Cambridge: Harvard University Press. 1982. Pp. vii. 347. \$25.00.

Dan P. Silverman proposes to examine the broadest ramifications of the financial and monetary crisis that engulfed the Western world after World War I. Earlier historians, he claims, have allowed the details of reparations to distract them from more fundamental issues. He selects for emphasis the crisis in economic theory that followed the breakdown of the classical gold standard, French and British fiscal and monetary policies and objectives in domestic debt management, Allied war-debt strategies, French attempts to tap American capital markets through J. P. Morgan & Co., and diplomatic efforts to reconstruct international trade. To create a new synthesis on these intricately related subjects would amount in any case to a tall order. It seems particularly ambitious for a study of modest length based almost exclusively on records of the British Treasury and French Finance Ministry and on the debt use of a half-dozen recent monographs. Silverman does not carry it off well, but as Samuel Johnson remarked in a different context, one is surprised to find it done at all.

Silverman has a shaky hold on facts and on the sequence in which they occurred. But he excels at self-promotion. We learn "for the first time," he declares, precisely how economists, bankers, and policy makers approached the crisis. What happened, in his view, was that the British and Americans ganged up on the French. Financial authorities in the two countries, abetted by the Dutch and the Swedes, believed in the superiority of the Anglo-Saxon race and preferred the Germanic economic system (whatever that may be). Dismissing Latin challenges as the reflection of inherent inferiority, they promoted fiscal and monetary orthodoxy as a stratagem to consolidate a system of Anglo-American financial and diplomatic hegemony. French leaders, quite reasonably, resisted. By no means ignorant about macroeconomics, they pursued throughout, both at home and abroad, policies of rational self-interest. Silverman, by escaping from a blinkered Anglo-American perspective, has grasped the essential truths that unaccountably eluded his predecessors over the past sixty years.

Very little evidence supports these melodramatic claims. It stands nonetheless to Silverman's credit

that he does not let a far-fetched thesis obstruct relatively accurate reporting of fiscal and monetary policies as they took shape in Britain and France between 1918 and 1922. The most solid chapter here describes the British debate about prolonging the Excess Profits Duty. Silverman rightly shows how the Treasury policies that prevailed in 1920–22 (a continued supertax on upper incomes, rejection of a supply-side tax cut, gradual conversion of the floating debt, a balanced budget, and slow deflation) reflected an implicit social bargain. He shows himself more muddled about France. He does provide data, however, confirming traditional views about the incoherence of the French budget-making process and the disastrous results of an uncontrolled deficit, one that rendered impossible the sharply deflationary monetary policy to which the authorities paid lip service. Silverman also offers a revealing account of the relations of J. P. Morgan & Co. with the French Finance Ministry directly after the war. He begins by hinting darkly that Morgan's gave unrealistic advice and made excessive profits, but concedes in the end, albeit grudgingly, that the firm did an excellent job in interpreting American securities markets during the postwar depression and in floating the 1920 and 1921 loans.

Silverman's treatment of diplomatic issues offers less to the informed reader. He fails to comprehend how the war-debt controversy became the chief vehicle for Anglo-American financial rivalry and does not grasp the point of the British argument that the debts should be set off against one another as if they all had equal worth. He takes the Amsterdam Memorial and the Brussels Monetary Conference of 1920 with more seriousness than they deserve and vastly overestimates the prospects for Anglo-Russian trade before the Genoa meeting of 1922.

Silverman's fundamental approach to European reconstruction rests on an apparent contradiction. He scores the poverty of traditional monetary notions, condemns the advocates of balanced budgets, and dismisses those who saw advantages in honoring domestic and international obligations within reason. He would have preferred fictitious credit creation and planned international inflation. At the same time, his analysis proceeds from the implicit assumption that somewhere between Liberty and Wall streets a magic golden fountain issues forth. One only needed a clever scheme to redirect the flow. The resolution of the contradiction comes in mysticism. Postwar stand-pat capitalism had become "intellectually bankrupt." Of Europe's interwar leadership, "only Mussolini, Hitler, and Stalin understood the fundamental role played by both nationalism and socialism in modern political systems. Fascism, National Socialism, and Communism offered the only 'alternative system' of economic orga-

nization" (p. 128). Readers who wish to extract useful material on budgets and debt management will have to bear with many such efforts at profundity.

STEPHEN A. SCHUKER
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CLAUDIO NATOLI. *La Terza Internazionale e il fascismo, 1919–1923: Proletariato di fabbrica e reazione industriale nel primo dopoguerra*. (Biblioteca di Storia, number 100.) Rome: Riuniti. 1982. Pp. 409. L. 19,000.

The initial poverty of the European Communist response to fascism has been a scandal and a source of trauma for years. Until the move toward a popular-front strategy in 1934, Communists consistently underestimated fascism and the damage it could do, partly because they were preoccupied with their rivalries on the left. Claudio Natoli does not alter the basic lines of this familiar story, but he accentuates its tragedy and anomaly by playing up the potential for a more forceful leftist response to early fascism in Italy. His most original passages document numerous attempts by workers to forge an anti-Fascist coalition on the local level—efforts that were ignored or actively impeded by Amadeo Bordiga and the Italian Communist majority, despite growing pressure from the Third International for an anti-Fascist alliance. In making Bordiga the villain, Natoli counters the recent tendency to look more kindly on the majority Communist leader, who has suffered so long in comparison with Antonio Gramsci. However, since the author never probes the reasons for the majority posture, his account does not add up to a satisfying interpretation, even though he presents a wealth of information in an attractive way.

At the crucial junctures, Natoli seems paralyzed by incredulity: the Communists' mistakes were apparently so egregious as to defy analysis. Ultimately, a form of hindsight dominates his account, precluding the imagination necessary to reconstruct the situation and to assess the protagonists' responses. Natoli fails to do justice to the unprecedented dilemmas facing the new Italian Communist party, as a minority on the left. Thus he implies that early fascism was an entity with an inevitable course of development programmed into it. In fact, however, the more imaginatively we grasp the situation of 1921, including the uncertain, contradictory nature of fascism at that point, the less obvious it is that the only plausible course for a Communist party was to subordinate everything to the battle against fascism. Moreover, to persuade us that superior strategies were available, the author needs a closer look at the potential allies, especially the *arditi del popolo*. Be-

cause Natoli never considers the political implications of Italy's war experience or the aspirations of the Fiume movement, he cannot fairly assess the Communists' reluctance to join forces with this heterodox grouping. Perhaps their concerns about contamination were not implausible.

In light of the dearth of systematic analysis in this account, it is never clear what the author intends to argue about the overall stakes and possibilities. Is he suggesting that fascism could have been defeated by a more concerted leftist response, or simply that it was unseemly for the Communists to have succumbed so readily? It is not obvious that a more forceful response would ultimately have proven more effective. Bordiga and his colleagues surely went wrong somewhere, but, from Natoli's account, we cannot tell whether the problem was theoretical confusion, inadequate assessment of the facts, or poor strategic conclusions drawn from those facts. Are we confronted with something anomalous—mere foolishness or blindness—or with honest errors of judgment in a situation full of dilemmas and risks? Without such structuring questions, Natoli's account becomes flabby and eventually wearies the reader. Nicos Poulantzas's more analytical treatment in *Fascism and Dictatorship* is preferable, even for those who do not accept his conclusions about the sources of the Communists' mistakes.

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Eastman School of Music
University of Rochester

SOLANGE GRAS AND CHRISTIAN GRAS. *La révolte des régions d'Europe occidentale de 1916 à nos jours*. Foreword by ROLAND MOUSNIER. Paris: Presses Universitaires de France. 1982. Pp. 263. 95 fr.

In this brief volume Solange Gras and Christian Gras provide a useful survey of the numerous minority nationalist movements that have emerged within many of the major nation-states of Western Europe during the past six decades. Their recurrent theme, lucidly expressed in a preface by Roland Mousnier, is that in the wake of the great bloodlettings of 1914–18 and 1939–45 these nation-states have entered into a period of crisis, many of them being menaced with dislocation as long-dormant but now-resurgent small "nations" within their frontiers seek to exploit their difficulties with demands for regional reform, autonomy, or even outright political independence.

The book is divided into two parts, flanked by a general introduction that establishes the dimensions of this so-called regional revolt against long-entrenched nation-states and a very brief conclusion in which the authors seek to gauge its significance for the future. The first part analyzes in successive

chapters the cultural, socioeconomic, and political components of regionalist action since 1916. The Grasés stress the primordial importance of the first of these though they acknowledge that the powerful Scottish nationalist movement owes almost nothing to the cultural component. In the second chapter they argue that most regional protest movements are fueled by socioeconomic grievances centering on the fact of local underdevelopment, an argument difficult to sustain in the cases of highly developed Catalonia and the Basque country, where ethnic national consciousness is intense. In any event, the authors not implausibly argue in the third chapter that it is the persistence of relative underdevelopment in ethnically distinct regions that has pushed most local protest movements generally leftward since 1945.

In the second part of their study the Grasés survey the vicissitudes of what they call the various "infranationalisms" that have emerged all over Western Europe since 1916. Their range is comprehensive and extends from discontented Lapps in northern Scandinavia to restive Andalusians and Sicilians in Mediterranean Europe. Most of their attention, however, is devoted to the Celtic revival in contemporary Scotland, Wales, and Brittany, the efforts of the Catalans in favor of regional autonomy, and the violent political acts of Basque and Corsican nationalists who demand political independence. Considerably less but still more than passing attention is likewise given to similar movements among the Flemings, Alsations, and Tyrolese.

Throughout, the Grasés argue that such regional protest movements based on ethnicity are here to stay and that central governments increasingly must take them into consideration if they are to remain viable. But they do not claim (the gloomy predictions of Mousnier notwithstanding) that the activities of any of these groups, except for the Flemings in Belgium, threaten the continued survival of any of Europe's existing nation-states. Even the Belgian exception probably does not mean very much, for Belgium since its foundation has always had something artificial about it, and the concept of a Belgian nation-state has never made much headway among the Walloon and Flemish communities who have lived under the authority of Brussels since 1830.

In any case, the Grasés, even if they are clearly hostile to the existing nation-state structure of Western Europe, do not look with equanimity on the proliferation of ethnically based regional protest movements and seem to agree with Mousnier that their triumph would mean the suicide of Europe (p. 10). Their hopes lie instead in the construction of a European federal state through negotiation by existing governments, whose historical fate it would then be to fade away. In such a new state, they imply, the rights and cultural identities of small ethnic commu-

nities would be guaranteed and regional disparities in socioeconomic development would be overcome. At the same time a new Europe would have been created that would be independent and powerful enough to compete on an equal footing with the American and Soviet superpowers. It is a beguiling dream, but still only a dream—as the Common Market's persistent failure to proceed toward this goal over the past decade has so amply demonstrated.

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E. A. WRIGLEY and R. S. SCHOFIELD. *The Population History of England, 1541–1871: A Reconstruction*. (Studies in Social and Demographic History.) Cambridge: Harvard University Press. 1982. Pp. xv, 779. \$60.00.

For anyone seriously interested in historical demography, this is an indispensable work. In several respects it transcends the earlier products of the Cambridge Group for the History of Population and Social Structure, with which the two authors are associated, as well as those of the prestigious school of Louis Henry and his colleagues in France.

We now see that the Cambridge Group's most important initiative was to organize a large number of volunteers whom its members trained to cull usable data from England's parish records. Of the almost 550 tabulations that were completed, 126 were rejected because the period covered was too short or because the record was too incomplete or inaccurate. The 404 tabulations that passed muster represented a total of approximately 3.7 million monthly totals of baptisms, burials, and marriages. Except that nothing was available for London or Monmouth County (presently part of Wales), the usable records covered most of England. Elaborate checks were made to adjust for underregistration and for differing starting and finishing dates. This sample, made as representative as possible, was then inflated to national frequencies, and baptisms were converted to births, burials to deaths.

The subsequent argument depends fundamentally on how well this reconstruction was made. The first 154 pages, supplemented by several of the sixteen appendixes, are essentially methodological, a detailed account of precisely what steps were taken and why. All the records were from Anglican parishes and thus left out diverse nonconformists, from Roman Catholics to Unitarians, whose numbers varied by region and period. Sharp rises in the number of burials were sometimes occasioned by local mortality crises, which had to be identified and approximately measured. In general, however, variation in registration was found to be fairly consistent

over the whole sample, and a computer program was devised to catch underregistration beyond that limit. In short, E. A. Wrigley and R. S. Schofield asked all the right questions, pondered aloud about a number of alternatives, and came up with what seem to me to be plausible answers. Every community study, whether of Crulai or of Middletown, implicitly relates to the larger society, but one must always wonder how representative really this village or small town was of the nation. To add up all of England's vital registrations over more than three centuries went well beyond any of the earlier generalizations.

The significance of vital registrations, however, can be grasped only against a known trend in population size, which provides a denominator for the calculation of birth and death rates. Using what demographers call the balancing equation, one can start with a base population and successively add births and subtract deaths; Ronald Lee devised a way of working this backward in time, which he called "inverse projection." The authors adapted this to include movement into and out of England, too significant a factor to be omitted; and with their "aggregative back projection" they derived quinquennial estimates of population size, age structure, and net migration. This methodological innovation, the key to the reconstruction of England's population growth, may be the work's most important contribution, as well as probably the future focus of a good deal of critical comment. "Net migration," for instance, does not mean what those words ordinarily convey; the figures are residuals, the difference in each quinquennium between observed and hypothetical birth cohorts. Virtually everything that a critic might say, however, the authors have themselves pointed out, and they add that an exaggerated skepticism about the results is also uncalled for.

How startling those results are can be conveyed succinctly by contrasting them with the familiar demographic transition, the model generally used to explain the growth of numbers during the modern period. According to that thesis, one can delineate three stages: (1) with high fertility and mortality, and a consequent slow population growth; (2) with continuing high fertility but falling mortality, and thus a sizable increase in population; and (3) with fertility falling to near parity with mortality, and something like zero population growth. Details of this schematic pattern have been challenged, but never on the scale of *The Population History of England*. According to Wrigley and Schofield's reconstruction, the highest expectation of life was from the mid-1560s to the mid-1580s, a figure not matched until 1871. This would seem to be unbelievable, but actually those who have hypothesized that the death rate fell substantially before the last

decades of the nineteenth century could find little substantiation in medical history. After rejecting all other possible causes, Thomas McKeown opted for a substantial improvement in sustenance. According to the reconstruction, much of England's population growth was the consequence of a rise in fertility—in flat contradiction of the conventional schema. What John Hajnal called the “European pattern” of family formation—that is, late marriage and a high proportion who never married—was eroded during these centuries. With persons marrying at earlier ages, they had more children and, paradoxically, also more illegitimate children.

In a chapter written by Ronald Lee, the trend in vital rates is put against what is known about fluctuations in the weather and in prices. Mortality was increased by a rise or fall in temperature outside the moderate range, but not by annual rainfall. Variation in prices affected mortality relatively slightly, usually with a lag of several years, but the effect on nuptiality was stronger. There was a strong association between marriages and births, as one would expect—but it was *not* due to first births, and the correlation is presumably spurious. These sometimes commonsensical, sometimes surprising results lead into a broader chapter on the economic setting of the long-term trends in fertility and mortality. A rise in real wages had little or no discernible effect on mortality; indeed, since sometimes the better wages were earned in a less healthful environment, the relation may occasionally have been negative. This absence of a significant link between economic conditions and mortality is contrasted with a marked association in the secular trends of real wages and fertility.

The work is summed up in an interesting chapter that begins with a model based on Malthus's positive and preventive checks and then successively adds complicating factors. Amazingly, the discussion of Malthus is based on the first (and preliminary) edition of the *Essay*, with no mention at all of *Principles of Political Economy*; it is only from such a truncated base that one could conclude, “It is characteristic of the Malthusian system that the factors affecting its operation are all endogenous to it”—that is, omitting the demand for labor, the price of food, and real income. This lapse apart, the chapter draws together the complicated argument of the book into a succinct summary that also invites the reader to speculate beyond its range. As Wrigley and Schofield note, the importance of their book goes well beyond the bounds of demography, for population is at the center of social and economic life.

This very brief summary of the main line of the book's thesis, however, considerably understates its range. Whenever it was possible to test one or another hypothesis or technique against other data,

this was done. As a prime instance, the “aggregative back projection” was tried out on the modern period and compared with census counts; the match was acceptably good. More generally, earlier works on the population also of other countries are continually brought in to illustrate or validate a particular point. Indeed, this book on the population of England covers much of the same material as the recent shorter and more readable work by Michael Flinn, *The European Demographic System, 1500–1820*, though with some markedly different conclusions.

WILLIAM PETERSEN
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AUDREY ECCLES. *Obstetrics and Gynaecology in Tudor and Stuart England*. Kent, Ohio: Kent State University Press. 1982. Pp. 145. \$18.00.

Some of the results of Audrey Eccles's fifteen-year study of English midwifery and related issues are presented in this slim volume. Her survey of the changes in obstetrical and gynecological practice from 1540 to 1740 suggests that Tudor-Stuart women, unlike their medieval and eighteenth-century counterparts, had the worst of two worlds: they received neither the sympathetic, “hands off” care provided by medieval midwives nor the informed clinical care of male midwives or physicians. If English obstetrical texts were not translations of Continental works, they were plagiarisms; if written by midwives, they showed a remarkable ignorance. Nearly all revered the ancient Greek physicians or Aristotle's biology, both of which in giving rise to humoral medicine, prevented any real scientific advance. The practical application to obstetrics of Harvey's discovery of the circulation of the blood in 1628, for example, was delayed for at least half a century. Most gynecological texts repeated the erroneous notion of the ancients that the liver was the “fountain of blood” (p. 21). Knowledge of the female reproductive system was limited. Though the ovum theory of conception was generally accepted by the mid-eighteenth century, the idea that a woman could not conceive unless she found pleasure in intercourse was still given credence. Confusion about the male and female genitalia, ignorance of the female menstrual cycle and of the physical changes of the uterus during pregnancy, among other misconceptions, prevented scientific medical practice. Hampered by ignorance, the false modesty of their patients, and the poor health of upper- and middle-class women, neither midwives nor male physicians could prevent frequent deaths in childbirth.

This might have been the definitive study of the subject had it not, like Tudor-Stuart women, fallen between two worlds: it is neither a popular nor

scholarly presentation. The chief fault is organizational, with much material out of chronological order. A discussion of gynecology rightly precedes that of sexuality and conception, pregnancy promotion and prevention before pregnancy diagnosis and the birth of the fetus. Brief chapters such as the first three might have been combined as they cover a mere twenty-five pages. The ovum theory was either discussed in Aristotle's *The Masterpiece* or it was not; the author contradicts herself (pp. 29, 45). Although the book has notes and a brief index, it lacks altogether a complete bibliography of the kind provided by Jane Donegan in *Women and Men Midwives* (1978). Most important, the author has not attempted to answer the question raised by her thesis: what accounts for the change in attitude toward obstetrical and gynecological practice during the Tudor-Stuart era?

ROSEMARY MASEK
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Las Vegas

DEWEY D. WALLACE, JR. *Puritans and Predestination: Grace in English Protestant Theology, 1525–1695*. (Studies in Religion.) Chapel Hill: University of North Carolina Press. 1982. Pp. xiii, 289. \$29.95.

Working from the premise that the concept of predestinarian grace in the Reformed tradition is a phenomenon of religious experience, Dewey D. Wallace, Jr., skillfully analyzes its development from England's first Protestants to the conflicting theological groups of the late seventeenth century. Because particular theological tenets reflect different kinds of piety, according to Wallace, the doctrine of predestination is a key to understanding the shifting religious patterns in early modern England. After demonstrating that a Reformed theology of grace stressing predestination emerged in England prior to 1558, Wallace convincingly shows that the Elizabethan Church of England was "uncompromisingly Protestant" (p. 28) and not the *via media* so often portrayed. Moreover, Wallace demolishes the recent attempt of extreme revisionists such as Patrick Collinson to paper over the differences between Puritans and Anglicans (*Conformists*) by underscoring the significance of their contrasting treatments of predestinarian grace. The Puritan theology of grace, grounded in "an inner and deeply existential core of piety" (p. xi), was characterized by a demonstrable fervor, warmth, and intensity, whereas the Anglican counterpart was more colored by humanism, Lutheranism, Erastianism, and conservatism. Elizabethan Anglicans were less interested than Puritans in the *ordo salutis*, preferring instead to emphasize sacramental piety, ritual, and a greater reliance on patristic authority.

Wallace is less sure of how to handle the Arminian challenge. On the one hand, he sees it, like Nicholas Tyacke, as a revolutionary force characterized by the introduction of doctrinal innovations. Conversely—and more appropriately—he regards Arminianism as a national Counter Reformation, a reactionary movement that returned for much of its inspiration to the traditional sacerdotalism of the medieval church. Wallace insightfully suggests that sectarian Arminianism, unlike its Anglican namesake, was evangelical in intent and therefore a precursor not of rationalism but of Methodism and the eighteenth-century awakenings.

For Wallace, Antinomianism is significant as the catalyst that led to the formation of theological parties in the mid- and late seventeenth century, a period in which differences between Anglicans and Nonconformists became increasingly theological. While the Anglicans were developing a rational, moral piety (as reflected, for example, in latitudinarianism), Congregationalists and particular Baptists came to espouse a "gracious piety" so rooted in the Reformed doctrine of predestinarian grace that it set them apart as high Calvinists. Afraid that this position was perilously close to Antinomianism, most Presbyterians modified predestinarian tenets and thus drew closer to the Arminians.

Thoroughly at home in the relevant primary and secondary literature, Wallace has written one of the most significant books on English Protestantism in this century. He has refuted Collinson's effort to blur the differences in the Elizabethan church, challenged Christopher Hill's attempt to make predestinarian theology a disciplining instrument of the emerging bourgeois society, and rejected Geoffrey Nuttall's endeavor to place Quakerism within the broad Puritan tradition because of its emphasis on the indwelling Spirit. Instead, Wallace demonstrates that the Reformed theology of grace, firmly rooted in experiential piety, was neither a "socially useful tool" of the bourgeoisie nor "primarily an element in an intellectual system" (p. 193), but the reduction of religion to the simplicity of the soul's confrontation with God. With a refreshing directness, Wallace has recaptured the essence of English Protestant teachings about predestinarian grace in the sixteenth and seventeenth centuries.

RICHARD L. GREAVES
Florida State University

KEITH LINDLEY. *Fenland Riots and the English Revolution*. London: Heinemann. 1982. Pp. ix, 276. £16.50.

The book examines the local response to the large-scale drainage schemes first seriously attempted in the fens of eastern England by entrepreneurs enjoy-

ing the patronage of Charles I. These projects, in which well-connected outsiders trampled the traditional patterns of land utilization and of communal property rights, were bitterly resented by the fenmen. Their resistance, a blend of representations to the authorities and lawsuits at Westminster with attacks on the drainers' engineering works, on their crops, and on tenants in the affected localities, has left a wealth of source materials: civil and criminal actions in a variety of courts, conciliar and parliamentary investigations, administrative records, polemical pamphlets. Keith Lindley's thorough mastery of these scattered sources is most impressive, and he deploys the information from them to provide a rich and complex narrative. Perhaps, however, it is too rich and complex. The book contains a superabundance of detailed information, but Lindley makes little attempt to provide a sustained analytical discussion of his subject.

Two general questions, treated superficially in the study, demand further consideration. The first concerns the economy and social structure of the region. Lindley's major contribution in this respect is to insist on the important part played by the local gentry in condoning, and even actively encouraging, the riots. Gentlemen were certainly involved, but the significance of their role cannot be evaluated without some examination of the general structure of fenland society and of traditions of political authority and deference within it. Other social questions, some suggested by contemporaries, also deserve discussions both in terms of the general structure of the society and of a prosopographical analysis of the rioters. Was resistance to the drainage most vigorous among the clique of wealthy yeomen that, as spokesmen for the drainers insisted, had previously grossly exploited the common pasture by overstocking? Were the "poorer sort" readier to act independently of their betters in periods of general economic crisis?

The second issue that merits further discussion concerns "political consciousness" among the fenland rioters. Lindley chastizes those historians who have argued that the fenmen, particularly at the outbreak of the civil war and in the aftermath of the execution of the king, were inspired by a visionary political and social ideology. He is right to insist that the evidence for this supposed radicalization is tainted, manufactured by the drainers in an effort to depict their opponents as dangerous subversives. But Lindley's valuable critique hardly warrants his more general conclusion that the fenmen "did not give expression to political feelings" (p. 65). Ideals about law, property, and governmental responsibility provided the basis for their actions in defense of their commons, and are as apparent in the rituals of riot as in their formal approaches to courts and councils. As the dispute dragged on, the

fenmen received an uncomfortable education in the ways of Whitehall and Westminster: their response combined moments of violent frustration with a growing legal and political sophistication. The cooperation with John Lilburne may not indicate that Leveller sentiment was rampant in the Isle of Axholme, but it surely suggests an unusual, if ultimately misguided, awareness of central politics among the yeomen and local lawyers who were fighting expropriation. Details of riots and threats, legal process, and parliamentary petitions are provided in abundance by Lindley; it is regrettable that they are not subjected to sustained analysis.

CLIVE HOLMES
Cornell University

ROBERT E. SULLIVAN. *John Toland and the Deist Controversy: A Study in Adaptations*. (Harvard Historical Studies, number 101.) Cambridge: Harvard University Press. 1982. Pp. viii, 355. \$27.50.

What pleasure John Toland (1670–1722) would have gotten from all the scholarly attention he has received in the past decade or so. There is a veritable industry in Italy devoted to the production of bibliographies and biographies about him—one largely inspired by Toland's admiration for Bruno—and now a fancy American university press has produced another biography. After his initial thrill, however, and despite his craving for fame and even notoriety, Toland would probably find little about himself he would recognize or little to be pleased about in this latest effort. Its author, Robert E. Sullivan, pronounces him a "failure" (p. 140), describes his writings as "mazes of inconsistencies" (p. 142), and charges him with considerable dependence on the arguments first developed by his enemies—the Anglican clerics and Unitarians who consistently attacked him during his lifetime and well beyond. In perhaps the final *coup de grace*, Sullivan urges the reader to look for "Toland's monument . . . in Georgian Anglicanism" (p. 277). This biography pursues a methodology that consistently interprets primary sources, as well as the intentions of their authors, from a set of assumptions that would have been foreign to them. Consequently Sullivan must find hidden meanings or exploit "contradictions" in order to sustain his own fixed assumptions about the nature of Christian belief and various Christian doctrines.

The moral to be extracted from Sullivan's tale is that liberal clerics in the end get what they deserve. Because they embraced that "Anglican distrust of any effort to establish Christian commitment on the basis of dogma" (p. 255), they grew lax on heretics. The result by the 1690s was a torrent of materialism, deism, and anti-Trinitarianism. Sullivan imag-

ines the late seventeenth- and eighteenth-century Anglican church as led by "broad minded churchmen" who were given to "anticlericalism" (p. 131) and who had in addition "repudiated Scholasticism without discovering a substitute and thus invited the wholesale revision of doctrine" (p. 139). Not even Toland, who seldom missed an opportunity to belittle some of these same clergy and who was in no sense their intellectual offspring (however illegitimate), could have grasped the full horror of what Sullivan describes as their "sustained tolerance . . . for incongruity." Yet no matter how naive Toland may have been with respect to the doctrinal and intellectual laxity of the clergy, he could never have written, as does this author when describing the sixteenth-century antecedents of liberal Protestantism: "Faustus Socinus' primary affinity with Erasmus lay in the assumption that the religion of Christ was ethical rather than intellectual" (p. 86). Toland simply understood historical context more subtly than that.

This sort of theological blacklisting may still have some place within the confines of conservative divinity schools, but it does nothing to advance historical understanding. In order to find what is useful in this otherwise very flawed monograph, the reviewer must try to ignore the perspective from which it is written. This task is rendered especially difficult because Sullivan's methodology, coupled with his convoluted prose, not only obscures the force of Toland's marvelous rhetoric, with its sustained assault on the pretensions of the established church, but also is directed against a number of scholars who have written about his politics and religion. In fairness it should be noted that I am among those scholars whom Sullivan attacks; in the process he also sometimes overstates my position (p. 284 n.) or distorts it (p. 114). Our most fundamental disagreement centers on Toland's influence, particularly on the Continent. I have assigned him a significant influence in certain circles (*The Radical Enlightenment* [1981]); Sullivan would assign him nothing of any consequence.

We should be grateful, I suppose, for having between hard covers a full account of Toland's year-to-year activities, as well as a very useful bibliography in English of his writings. Neither I nor Toland's Italian biographer, Chiara Giuntini, accepts one early tract, *Two Essays Sent in a Letter from Oxford* (1695), as being by Toland, although it is a pity that Sullivan does not argue his case better as the tract would further document Toland's debt to the new science and to Hobbes. But to this reader its flat style does not seem to match the confidence and pugnacity found in *Christianity not Mystical* (1696, but written in 1694–95).

This failure to develop a case where one would prove very interesting extends to Sullivan's treat-

ment of Toland's various involvements in secret societies. Sullivan rejects any ties to early Freemasonry. Rather than trying to come to terms with the evidence that exists, in particular Toland's links with Rousset de Missy and hence with the leader of Amsterdam Freemasonry, Sullivan sidesteps the issue by claiming that Toland was too recalcitrant "to submit to the discipline of prolonged membership in any organization dedicated to an ideology" (p. 203). Sullivan thereby adds recalcitrance to the list of his subject's failings, which includes "truculence," "adolescent playfulness," "paradoxicalness," and, not least, a "reluctance to pursue step-by-step institutional reform" (p. 157). It is never possible to extract from Sullivan's ambivalence toward his subject why Toland should be studied at all. Incidentally, there is one minor factual error: the English translation of Bruno's *Spaccio* appeared in 1713, not in 1712 (p. 199).

Sullivan's monograph is a useful reference tool, not very readable and not right, but on balance better published than not.

MARGARET C. JACOB
University of Leiden

LINDA COLLEY. *In Defiance of Oligarchy: The Tory Party, 1714–60*. New York: Cambridge University Press. 1982. Pp. viii, 375. \$45.00.

With this publication, the Cambridge University Press, coincidentally, marks the arrival on this side of the Atlantic of the most recent recruit to the ranks of Cambridge-trained early-modern English historians now teaching in the U.S. Happily, Linda Colley's book more than fulfills the promise of a good but somewhat narrower dissertation. Students of the period have long been aware of the limitations of Keith Feiling's 1938 account of the "second" Tory party. Nor has Eveline Cruickshanks's depiction of the post-1714 Tories as Jacobites convinced. Now, and for the first time, we have an in-depth examination of Toryism between the reigns of Anne and George III.

Combining topical and narrative chapters, Colley presents a well-researched case for two conclusions. First, that despite proscription, leadership crises, and internal differences, the Tory party had a continuous, if not always effective, existence at the center and in many constituencies under the first two Hanoverians. An important corollary is that the alliance with the dissident Whigs between 1727 and 1741 never blurred significantly the Tories' partisan identity. "Country" platforms were useful to the anti-Walpolean coalition, but other underlying Tory values and attitudes persisted—especially commitment to a view of society postulating the need for an

identity between the interests of the monarchy and those of the Church of England.

The author's second main conclusion is that the Tories, despite the long years of exclusion from the commissions of the peace, remained more politically potent in the localities and more politically attractive to the electorate than their limited numbers in St. Stephen's Chapel would suggest. In Colley's view, it was the entrenchment of the Court-backed Whig electoral patrons in the narrow constituencies that was a prime factor in the Tories' failure ever to win a majority in the Commons after Anne's demise. Indeed, she reckons that in terms of the number of votes cast the Tories won the 1722, 1734, and 1741 general elections. At the same time, she delineates skillfully the dilemma that the Tories' electoral position posed for them—at once dependent upon popular support, especially in the larger boroughs (and utilizing tactics such as the mobilization of constituency "instructions" to MPs), and wary of popular radicalism. Under these circumstances, it is noteworthy that at least a minority of Tories were prepared from the mid-1730s to advocate a redistribution of parliamentary seats, though few could be found to champion an extension of the franchise.

Although Colley casts doubt upon the significance of 1760 as a watershed in eighteenth-century political development, she is even more eager to challenge the conventional characterization of the 1715–60 period as one of unusual sociopolitical stability. Certainly, she has demonstrated the vitality of the Tory party under the first two Hanoverians, and her point that the Whig proscription of so perdurable a political element was itself potentially destabilizing is well taken. In addition, she goes beyond her own research to argue (in a markedly revisionist opening chapter) that the Glorious Revolution dissolved the seventeenth-century tension between the monarchy and the landed elite so that the Whig-Tory clash between 1689 and 1714 "was not at base a source of political instability," but rather a "stylised, often ruthless, conflict which took place within a social consensus" (p. 12). Perhaps, then, we may look forward to a revision of Feiling's 1924 study of the "first" Tory party as her next contribution to the field.

HENRY HORWITZ
University of Iowa

J. ANN HONE. *For the Cause of Truth: Radicalism in London, 1796–1821*. (Oxford Historical Monographs.) New York: Clarendon Press of Oxford University Press. 1982. Pp. 412. \$45.00.

Popular ferment in London has been an intermittent cause of concern to the modern British establishment from the High Church crowds of 1715 to

Ken Livingstone's left-wing agitation of the 1980s. J. Ann Hone in this tightly argued work discusses radicalism in the capital from the disturbance surrounding the passage of the repressive Two Acts in 1795 to the turbulence of Queen Caroline's funeral procession in 1821.

For the Cause of Truth is an extraordinarily well-researched and reasonably well-written book containing a wealth of information on the golden age of British radicalism. No future scholar of the period can afford to ignore it; though the absence from the bibliography of two important works, Ronald Huch's *The Radical Lord Radnor*, and Arnold Harvey's *Britain in the Early Nineteenth Century*, stands out. Regrettably, Hone in two areas fails to give her subject a fitting focus. First, the author's radicals are lumped together with few ideological components in sight. Tory radicals and philosophical radicals, Owenites and Paineites, hardly make an appearance here. This is intentional, and it enables Hone to focus more on those considerable ties that united radicals as opposed to the secondary considerations that divided them. Yet, for this reader at least, to eschew ideological criteria altogether confuses more than it enlightens. Second, she presents the book as a "local study" (p. 6), a genre, after all, for which a considerable bibliography now exists. Yet, in some ways this is a misnomer, and readers should be forewarned that the book is not a social study of London's radicals. No attempt is made to analyze their social background or occupational status; to investigate their political relationships with the governing bodies of shire or borough; or to utilize, where possible, the voting lists. Indeed, London is almost incidental to the main story. And to entirely ignore geographical and institutional situations creates an odd imbalance in the work.

The most important section of the book is the first half, which carries the story of the radicals to 1809. Hone believes in the continued consequence of British radicalism even after Pitt's repressive legislation. Quite rightly, she will have no truck with those historians who have been allowed to dismiss that activity in 1796, only to resume the story in 1809. More controversial is her tentatively offered thesis about cooperation between radicals and Foxites during the 1790s. While she agrees with E. P. Thompson that there really was a revolutionary conspiratorial movement, she expands the participating social classes upward until one reaches the level of seditious dukes. Part of the importance of her thesis lies in the rehabilitation of the reliability of government spies and intelligence agencies. Hone only wishes that these competent spies had been allowed to infiltrate the great Foxite country houses to search for traitors. This may overestimate the radical inclination of the Foxite-Whigs. To take one example, was James Mackintosh really a "young and

radically inclined barrister" in 1796 (p. 23)? He may indeed have taken to visiting Thomas Holcroft, as Hone shows, but he was also visiting Burke at Beaconsfield, more or less repudiating the *Vindiciae Gallicae*, and in some ways prefiguring that union between her seditious Foxites and Burke's political heirs that was shortly to occur.

JAMES J. SACK
University of Illinois at Chicago

DON LOCKE. *A Fantasy of Reason: The Life and Thought of William Godwin*. Boston: Routledge and Kegan Paul. 1980. Pp. xi, 398. \$30.00.

Don Locke is an analytical philosopher who has written extensively on philosophical psychology and epistemology and who holds a chair of philosophy at the University of Warwick. His new biography of William Godwin is in no sense a work of philosophy, but it shows very nicely the advantages of writing the intellectual biography of a major thinker from the viewpoint of a modern philosopher. Although we do now have a convenient, inexpensive text of the *Enquiry Concerning the Principles of Political Justice* (1976), and although Godwin naturally figures prominently in the numerous recent studies of the lives of Percy Bysshe Shelley and Mary Wollstonecraft, he has received no remotely satisfactory modern biography. *A Fantasy of Reason* certainly repairs this omission.

What it offers is not a full-scale life of a figure whose manuscript relics are still difficult to use, whose masterpiece has yet to receive a really adequate modern edition, and whose thought engaged a bewildering variety of theoretical issues. Instead, Locke sets himself the more literary objectives of conveying the sense of Godwin's sad and harassed life, while attempting also to do full justice to the drama, importance, and (intermittent) force of his thinking. In both of these goals he is remarkably successful. *A Fantasy of Reason* does not quite have the *éclat* of Richard Holmes's marvelous *Shelley: The Pursuit* (1974). But it is a warm, witty, and deeply sympathetic portrait of a man whom, in a more superficial and disdainful study, it would be easy to find markedly unattractive. It conveys, too, a shrewd and remarkably clear picture of the boldness and integrity of Godwin's initial conception of the requirements for human freedom and an equally clear, and rather touching, portrait of the steady erosion of this conception under the weight of personal experience and political reconsideration. Although he is writing of a thinker who often appears ludicrously naive, the quality of Locke's own intellectual judgment is as impressive as his fair-mindedness.

One striking moral of Locke's discussion (not

drawn by Locke himself) is the close parallel between Godwin's confrontation of the political and theoretical dilemmas of individual freedom and reason and the vicissitudes of the Frankfurt school of critical theorists (for example, compare pp. 75 and 107 with Raymond Guess's admirable *The Idea of a Critical Theory* [1981], chap. 3). If Godwin often seems naive, this may well be simply because he was unwise enough to state clearly what a great many modern moralists have presumed altogether more shiftily. A second moral, of a more tractably academic character, is the potential fascination of a number of further lines of research into the provenance of Godwin's thought and its relations, both lineal and disjunctive, with that of his British and French predecessors, both religious and secular.

Godwin's life and thought were in many respects quite genuinely absurd. But the quixotic aspects of his thought are distressingly hard to separate from the quixotism of many of our own more cherished beliefs.

JOHN DUNN
King's College
Cambridge University

JAMES K. HOPKINS. *A Woman to Deliver Her People: Joanna Southcott and English Millenarianism in an Era of Revolution*. (Dan Danciger Publication Series.) Austin: University of Texas Press. 1982. Pp. xxii, 304. \$30.00.

A contemporary likened the Southcottian movement to the Fifth Monarchist zealots of the seventeenth century. Though the comparison was unfair, the career of Joanna Southcott has much of the flavor of the Civil War period. Born in 1750, the daughter of a Devonshire tenant farmer, she claimed that God had chosen her to declare His imminent return to earth. Her followers, many thousands, formed a movement that lasted into the present century.

In the first full and scholarly account of Southcott's life, James K. Hopkins gives us a sympathetic and well-judged interpretation. He evokes the atmosphere of her childhood home, her close but strained relationships with her parents, and her early loves—all described in some detail in her own prolific writings. Fascinated and repelled by sexual love, Southcott found her ideal role as the Bride of Christ as it was announced to her in a vision in 1792. In the next few years she achieved a considerable local notoriety, and Hopkins relates her appeal to a traditional popular culture in which prophecies, witchcraft, astrology, and magic still flourished. She prophesied about the weather, the harvest, politics, and the deaths of local celebrities; and her successes gave weight to her religious claims—much as in the

early career of the Quaker George Fox. She had no new theology to unfold and hoped to work within the Methodist or Anglican establishments. Rejected by both, she was pushed inevitably into separatism. Her activities took on a national significance in 1801, when she first published her experiences in print—the first of sixty-five books and pamphlets. Adopted by a group of clerics and professional people, she was taken to London; and a national movement sprang up almost overnight with preachers ministering in Southcottian chapels. To the faithful, many of them artisans or unmarried domestic servants, Southcott offered security. Her “seals” promised protection if the French invaded, a place of honor when Christ returned, and a guarantee of salvation. In a country undergoing industrialization, with low wages and high food prices, the force of her appeal is hardly surprising. She left the details of the millennium vague. Rich and poor would live harmoniously in a system of idealized paternalism, though Southcott spoke more sharply when the rich ignored her message. In 1813, at the age of sixty-four, she announced herself pregnant by the Holy Spirit. The child would pave the way for Christ’s return, and her followers awaited the event with renewed excitement. The diagnosis was alas mistaken, and Southcott was in fact dying. The faithful defiantly declared after her death that there had been a spiritual birth, but the importance of the movement was over.

The outlines of Hopkins’s analysis are not dissimilar to those of J. F. C. Harrison, and the two seem to have exercised a mutual influence. Both discuss the links between millennial excitement and the secular radicalism of the 1790s. Hopkins argues that the reformers’ ideology came from a wide variety of sources. Some of Southcott’s followers were far more radical than their leader. One dreamed of a society reduced to a “pleasant plane of equality” (p. 197). Another, the engraver William Sharp, was a close associate of Horne Tooke, tried for treason in 1794. As in the previous century, the lines dividing religious and secular visionaries were blurred.

BERNARD CAPP
University of Warwick

JAMES EPSTEIN. *The Lion of Freedom: Feargus O'Connor and the Chartist Movement, 1832–1842*. (Croom Helm Social History.) London: Croom Helm; distributed by Biblio Distribution Center, Totowa, N.J. 1982. Pp. 327. \$33.00.

Long-maligned Feargus O'Connor has needed a rehabilitative biography for some time. James Epstein’s study has accomplished some of it; but the subtitle should stand as the only title, because the

book is essentially an examination of the interplay of O'Connor’s leadership in a key decade. Along the way the author retells much of the history of Chartism, with emphasis on its national and class dimensions. Since Epstein seeks to explain Chartism and O'Connor at the same time, much of his effort is expended on what have become obscure disputes, policies, leaders, and organizations. He handles them all in a clear, cold, factual, and analytical style. Therefore, it is no wonder that most of O'Connor’s blood, color, humor, and Irish blarney have been drained out of him.

Even so, Epstein’s sympathetic and critical assessment serves as a long-needed corrective to the aspersions of Chartist craftsmen who became authors, such as R. G. Gammage and William Lovett, and some of the later craftsmen of academe, such as Mark Hovell. Epstein has replaced O'Connor the egotistical buffoon with an O'Connor who is something of the Lenin of Chartism. Feargus O'Connor emerges as a “steady and trusted” leader (p. 8) who “radiated enthusiasm and confidence” (p. 195) and projected “a consistent programme of action” (p. 155). O'Connor was “a master of the theatre of popular protest” (p. 268). His “intelligent and consistent leadership” (p. 314) always bore “intransigent class tone and class perspective” (p. 264). Moreover, O'Connor was always “clothed in personal sacrifice and unquestionable integrity of motive” (p. 146).

The author denies that O'Connor was a Tory radical, that he really courted a middle-class alliance, that he had a weak base in southern England, that he was dictatorial, or that he was an opportunist. Epstein admits that O'Connor’s ideological development was not extensive, but insists that he must be placed in a proper historical context. He sees O'Connor as a transitional figure standing between the “tradition of independent gentlemanly leadership” (p. 235) and the democratic leadership of a modern workers’ party. Such a context corrects what Epstein calls the most misunderstood aspects of his leadership, its “personalized” approach and “paternalistic tone” (p. 90). Despite his vast appeal as a “charismatic demagogue” (p. 5), O'Connor was fundamentally a constitutionalist who used the rhetoric of violence with considerable skill.

Above all, Epstein praises O'Connor for creating and sustaining working-class unity in a national challenge to the established order by channeling the raw energy of Chartist protest into durable institutions, specifically, the National Charter Association, its Executive Committee, and the whole apparatus of the *Northern Star*. Epstein places the blame for the failure to carry the Chartist points upon the strength of the opposition rather than upon any weaknesses of O'Connor, who has long served as a scapegoat.

This vigorous revisionism is testimony to the

dominance of E. P. Thompson and Dorothy Thompson over the younger historians of Chartism, who tend to be firmly entrenched on the class-conscious left. The former is quoted throughout as gospel, and the latter supervised a version of this study as a Birmingham University Ph.D. thesis. Feargus O'Connor still needs a full biography, but whoever writes it will be indebted to Epstein for providing this rehabilitation of ten years of his political career.

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CHRIS FISHER. *Custom, Work, and Market Capitalism: The Forest of Dean Colliers, 1788–1888*. London: Croom Helm; distributed by Biblio Distribution Center, Totowa, N.J. 1981. Pp. xvi, 203. \$27.00.

During the latter part of the nineteenth century, while F. W. Maitland and Sir Henry Maine were theorizing the passages from status to contract and from customary to statutory law, many groups of Englishmen were still living those transitions. The changes were experienced often as personal traumas and collective tragedies. The process of enclosure, the destruction of traditional agriculture, and the growth of industry all began well before the nineteenth century, of course. What that century saw most clearly was the gradual elimination, or transmutation, of the traditional communities wherein common people resisted those changes they did not want. Chris Fisher's *Custom, Work, and Market Capitalism* is a chronicle of this transition in the Forest of Dean.

"Time immemorial," for defenders of customary rights in this West Gloucester Crown Forest, as in much of the rest of England, dated essentially from the eighteenth century. During that period of gradually increasing population, urbanization, and demand for coal, a community of "free miners" had grown up in the forest. These miners were free in the sense of their independent access to their livelihood. They had no formal title to their mines, but simply a customary right born of long usage. It is possible, but uncertain, whether it would have stood a legal test. The miners claimed other rights, however, that were certainly beyond any legal guarantee—they encroached upon the crown lands, built houses, and turned their livestock free to forage in the commons that, if they were of legal right open to anyone at all, were open only to the residents of neighboring parishes, not to the foresters. Custom was in conflict even with "customary" common law.

A mine court arbitrated among the free miners and protected their customary right to exclusive access to the forest's mines until 1832, when it became embroiled in a series of personal disputes

and dissolved itself. Underlying this and other changes were new technologies, new market pressures, and rising population. The miners, nonetheless, were able to stymie the efforts of the crown to "regularize" titles and relationships. In 1838, an act of Parliament gave them formal title. Even this was transformative; customary rights were "secured" by making them statutory. In no other area could the foresters defend themselves even that much. The monetary power of outside capitalists—"foreigners," as the foresters called them—transformed mining and reduced most of the formerly free miners to day laborers. Old cooperative patterns of work were dissolved; new gradations of status and wealth were introduced. Still, sheep continued to graze on the commons; no government ever felt sufficient confidence or interest to fully transform the legal and productive basis of local life.

Fisher tells this story tersely in a well-researched, but resolutely narrow, book. Near the end he remarks that "the Forest was not separate, in the nineteenth century, from the forces which were at work in British society at large" (p. 172). His book does little, however, to relate the popular activity it chronicles to other locales or to consider its connections to reform politics, Chartism, or changing patterns of protest and collective action. The nature and outcome of the free miners' struggles were, in fact, bound up with politics, economics, and social change on a much larger scale. They are significant not just in themselves, but as an instance of the disruptions that affected trade after trade through the century. Had Fisher looked at some of this larger history and learned from some other case studies, he might have looked more deeply into his own material and been able to offer conclusions of more general interest.

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KENNETH D. BROWN. *The English Labour Movement, 1700–1951*. New York: St. Martin's Press. 1982. Pp. 322. \$27.50.

Kenneth D. Brown has set himself a most ambitious task in presenting the reader with an outline of the development of working-class organization in England since the Industrial Revolution by synthesizing much recent historical research (see the preface). The book begins with an introductory chapter by L. A. Clarkson, a colleague of the author at Queen's University, Belfast, that provides an informed and succinct survey of the emergence of wage labor in Britain during the period 1500–1800. What follows is essentially a collection of bibliographical essays on

some of the major debates in British labor history, beginning with a critique of the Webbs' treatment of early trade union history and moving to more recent themes, including the making of the English working class in the years between 1780 and 1832; the reasons for the reputed acquiescence of British labor during the mid-Victorian period; and an assessment of the extent to which labor unrest posed a challenge to the established social order during the crucial 1900–21 period. The book ends with a brief postscript on the years 1939–51, which culminated in the election of the Attlee Labour government.

The reader is taken briskly through the recent history of the *writing* of British labor history, as much as the history of the animal itself. While the undergraduate student will doubtless be assisted thereby in coming to grips with the voluminous literature that has appeared on this subject in recent years, including the author's own writing on topics like the applicability of the concept of social control in explaining the ebbs and flows of labor unrest, the reader is soon made aware that Brown's attempt at a synthesis of recent historical research rests on the particular philosophical perspective he adopts. The latter consists of a combination of the view that the British labor movement has been inherently moderate in character, despite significant periods of discontinuity and rising militancy, like the period 1875–1900 (which the author himself recognizes), with a teleological analytic that culminates in British labor finally reaching the "Promised Land" under the Attlee Labour government. Although Brown moves easily among the economic, social, political, and cultural factors shaping the history of the British labor movement, there is little grasp of the dynamic *interaction* of these factors, especially in light of labor's changing relations with other social groups and institutions like employers and the state, which in recent years have become of increasing interest to scholars. Moreover, there is a paradox in Brown's overall interpretation—and particularly his argument for continuity rather than discontinuity in British labor history—in that it is more profoundly *ahistorical* than some of the interpretations he chooses to criticize, especially if the latter are deemed Marxist. This is evident, for example, in the author's treatment of labor's collapse during the 1921–27 period, following the emergence of the years 1900–21, where the causes, extent, and trajectory of the former are very thinly developed.

Well-researched but disappointing as an interpretation is the impression left by this book, which is surprising given Brown's established record of scholarship in the field. A final note to the intended reader of this book—or perhaps its publisher. It is entitled *The English Labour Movement* whereas its contents are clearly British in scope. John MacLean

would turn in his grave if he knew he had figured in a history of the English labor movement.

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J. A. BANKS. *Victorian Values: Secularism and the Size of Families*. Boston: Routledge and Kegan Paul. 1981. Pp. ix, 203. \$25.00.

In 1954 J. A. Banks published *Prosperity and Parenthood*, an analysis of the English fertility transition that is still widely read and highly regarded. In it Banks explored the socioeconomic roots of Victorian middle-class rationality, a rationality that first promoted the postponement or avoidance of imprudent marriage in the 1850s and 60s, and ultimately the adoption of fertility control in the late nineteenth century. Family limitation, Banks argued, was a defense within marriage against the possible erosion of living standards in a generally pessimistic climate exacerbated by a servant shortage.

Although the book received a very favorable reception from most social historians, the rise of demographic history has gradually led to an increasingly critical re-evaluation of its methodological and conceptual limitations. Partly in response to cumulative criticism Banks seems to have revised *Prosperity and Parenthood*, but since the revisions are so extensive and cover much new material his latest analysis of Victorian family limitation has been given a new name—*Victorian Values: Secularism and the Size of Families*.

In this latest reworking of the explanation for the adoption of fertility control among the Victorian middle classes, Banks considers whether or not the decline of religion (as the reciprocal of the rise of a secular mentality among the middle classes) influenced the collective propensity to adopt family limitation in the face of continued opposition to it by Christian moralists. The author concludes that the middle classes remained surprisingly faithful to traditional religion and hence their singular apostasy in this one area becomes all the more puzzling.

It is at this point that Banks begins to recast the arguments in *Prosperity and Parenthood* as regards the social and economic pressures on the middle classes. Middle-class families, it seems, were driven to adopt fertility control against their religious predispositions, not because both children and servants were becoming increasingly expensive, but because parents were under a steadily mounting pressure to raise their sons as successful competitors in England's evolving meritocracy. Rational short-run calculations about the costs of childbearing were overshadowed by rational long-run calculations about

how many sons could be successfully equipped with the background and education necessary to perform well in the competitive examinations that constituted the gateway to socially prestigious and well-paid positions (p. 137). In *Victorian Values* Banks seems to argue that the psychological pressures involved in outfitting sons for the obstacle race were equal to or more important than financial pressures in motivating the adoption of fertility control.

Banks's arguments, as always, are well conceived and forcefully argued. But, as the author himself insists, they rest on circumstantial rather than direct evidence; thus, the only way to verify them is to eliminate the main alternative explanations. That his efforts to do so constitute the weakest part of the book should not be surprising. It is as difficult for Banks as for most involved scholars to construct the strongest possible case for arguments he wishes to demolish. Thus key concepts like "secularism" or "feminism" are generally defined and elaborated upon in ways that make them not quite but almost straw men (straw people?) and hence relatively easy to eliminate.

Anyone hoping that in the intervening years between *Prosperity and Parenthood* and *Victorian Values* Banks would have completely transcended the weaknesses of the earlier version will probably be disappointed. In both versions Banks is still trying to analyze the causes of fertility change by class over time, without first analyzing the nature and limitations of the differential fertility data available and the technological complexities of measuring fertility change in the short run. (It seems probable at this point that English fertility began to fall as early as 1811 or so and that this fall was interrupted by a mid-Victorian plateau before resuming its decline in the mid-1870s. Because the early decline occurred at the national level it almost surely involved many working-class families. See E. A. Wrigley and R. S. Schofield, *The Population History of England, 1541-1871* [1981] p. 235.) In addition, in both books Banks treats the British fertility transition as something that can be explained almost exclusively with reference to British social and economic history, although the same transition was proceeding among all Western European and overseas European populations (this insularity is somewhat modified in the later book). Finally, while Banks assumes that there were clear-cut lead/lag relationships among social classes (with the middle classes leading the transition), this point can be disputed. Once again, depending on how secular (long-range) fertility is measured, a case can be made for the parallel adoption of fertility control among some segments of the working classes, although at a reduced level of intensity. Although Banks gives us a very interesting discussion of reactions of the working class to the

rise of an educationally based meritocratic system, he neglects the evidence relevant to the history of working-class fertility strategies. (Angus McLaren's *Birth Control in Nineteenth Century England* [1978] is the best guide to the nontechnical evidence relating to working-class fertility control.)

Nevertheless, all those who welcomed *Prosperity and Parenthood* as an outstanding example of sociological analysis applied to the explanation of fertility change will find it equally imperative to read and consider the arguments in *Victorian Values*.

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VIRGINIA BERRIDGE and GRIFFITH EDWARDS. *Opium and the People: Opiate Use in Nineteenth-Century England*. New York: St. Martin's Press or Allen Lane, London. 1981. Pp. xxx, 369. \$25.00.

British interest in the history of domestic opium use and controls was meager until many young heroin addicts emerged in the 1960s and focused attention on a previously minor social problem. Despite acknowledged difficulties in determining opium and morphine consumption, Virginia Berridge, a social historian, and Griffith Edwards, an addiction specialist, have made a major contribution to the history of drug abuse policy and have swept away some cherished misconceptions.

Opium and the People is substantially limited to the nineteenth century, except for the final chapter by Edwards, which thoughtfully surveys contemporary addiction conditions and policies in light of the preceding history. The remainder of the book documents the casual use of opium early in the century as a tonic, a preventative against fevers (chiefly in the Fens), a soothing medication for infants, and as a means of suicide. The attempt to define opium use as a professional health responsibility and to consider its habitual use a disease is a central theme. The stimulus for control is largely ascribed to class tensions. The working class's consumption was described as being misperceived by higher classes as an unmitigated evil, although the authors claim that there were some positive features in the dosing of fretful infants given the appalling family conditions wrought by the Industrial Revolution. Nevertheless, they grant that infant deaths were a legitimate cause for worry, and that the first major law to control access to opium, the Pharmacy Act of 1868, had a dramatic effect on infant mortality from opium overdoses. Although the evidence for class tensions is somewhat strained, the account of professional

competition that divided the drug market in the latter half of the century between physicians and pharmacists is persuasive. The moderate controls (by today's standards) of the Pharmacy Act were followed by a gradual decline in the casual, non-medical use of opium through the remainder of the century, so that by the 1920s authorization to provide heroin and morphine to some addicts, the "British system," was "witness to the small scale of the problem rather than the cause of that scale" (p. 255).

Berridge and Edwards find that several now-familiar drugs had little popularity in Great Britain until recent decades. The infamous Chinese opium dens of London's East End existed more in myth than in reality. Recreational use of cocaine was almost unknown in 1900, and heroin use is described as minimal in 1920 when, as a requirement of the Hague Opium Convention, Britain enacted the Dangerous Drugs Act. The authors could well have emphasized the benefit to Britain of having in place formal channels of drug distribution prior to the massive international production of cocaine after 1884 and of heroin after 1898. It would have been desirable to have had a more detailed analysis of the British morphine manufacturers and exporters, which were among the largest in the world. Yet, this is a pioneer study that will surely encourage further research in many areas. The authors have succeeded in making available the history of a controversial social problem, while neither fitting their conclusions to current addiction ideologies nor prescribing a "solution" drawn from history.

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PERRY BUTLER. *Gladstone: Church, State, and Tractarianism; A Study of His Religious Ideas and Attitudes, 1809-1859*. (Oxford Historical Monographs.) New York: Clarendon Press of Oxford University Press. 1982. Pp. 245. \$42.00.

"Religion was central to Gladstone's life." Perry Butler's statement is a commonplace among historians; it is his task to show that the commonplace is not trite, to invest it with reality by a close examination of the relation between Gladstone's religious ideas and his politics in his first fifty years. Despite the general recognition of the importance of the subject, it has not received monographic examination until now, with the partial exception of Alec Vidler's *The Orb and the Cross* (1945). Considering the copiousness of the sources, exhaustively examined, and the anfractuosity of Gladstone's reasoning, painfully traced, this is an ambitious project for an Anglican curate's doctoral dissertation; but he

does it rather well in this sound, thorough, yet relatively brief book.

Butler organizes his study under three chronologically overlapping themes, which makes for a certain repetitiveness. The first, "the making of a High Church Tory," shows how the crisis of the Reform Bill gave Gladstone a religious vocation as a politician in the service of the Church of England through the Tory party. This was paralleled by the modification of his original evangelicalism through his apprehension of the corporate and sacramental character of the church. The second (and most important) section starts by examining Gladstone's statement, in *The State in Its Relations with the Church* (1838), that the ideal state has a religious conscience and Christianizes the social order. Butler then shows how this ideal shattered in the 1840s as Gladstone realized its impracticality and the unfitness of the Tory party; the Maynooth crisis of 1845 was decisive. Gladstone had to reconstruct not merely his politics but his vocation, which he did through his growing acceptance of the principle of liberty: the church was to be served by working for its freedom (not disestablishment) in order that it might maintain Catholic truth. This concept of liberty, which was consistently applied to other denominations and legitimized the pursuit of social justice as a merely political ideal, was the key to making Gladstone a High Church Liberal.

The third section of the book turns back to trace Gladstone's relations with the Oxford Movement. Gladstone's development of a High Church position was largely idiosyncratic; he was virtually uninfluenced by the Tractarians, whom he nonetheless generally supported. Butler finds a touchstone of Gladstone's position in his friendship with two other non-Tractarian High Churchmen, Henry Edward Manning and James Hope, who both converted to Roman Catholicism in 1851. Gladstone's continuing Anglicanism is attributed largely to his strong anti-Roman feelings and his "unquenchable optimism" about the mission of the Church of England. Butler hints that there may have been a certain ambiguity here, as in Gladstone's political readjustment. One wishes that the story had been carried into the 1860s, when the tension between the ideal and the practical, between Catholicism and liberalism, produced, as G. I. T. Machin has shown, its most fruitful results.

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F. B. SMITH. *Florence Nightingale: Reputation and Power*. New York: St. Martin's Press. 1982. Pp. xii, 216. \$25.00.

There are, of course, many biographies of Florence Nightingale. The standard, two-volume work by Edward Cook appeared in 1913, the more popular but flawed account by Cecil Woodham-Smith in 1950. So why is another needed? F. B. Smith's study is unique in both scope and interpretation. As the subtitle suggests, it is a study of power and reputation rather than a full-fledged biography. In successive chapters Smith considers the periods of Nightingale's public activity: at the Invalid Gentlewomen's Institution, Harley Street, London; in the Crimea; and behind the scenes in London during the sittings of the Royal Commissions, which she helped create and manage, on the sanitary condition of the army and the sanitary condition of India. There is also a chapter on her dealings with the nursing reform movement and a final chapter that discusses her views on a number of other issues.

A modern reader of the Nightingale papers soon realizes that this Victorian heroine was dogmatic, manipulative, and tough. Using her papers and those of the persons and institutions with whom she dealt, Smith describes at length her intrigue and opportunism and assesses the common notions of her accomplishments. Particularly telling in the latter regard is his comparison of her claims of success at Harley Street and in the army hospital in the Crimean War zone with the hospital records from her period of tenure. Smith's portrait shows the woman a failure at practical nursing (unable to choose or manage subordinates, unwilling to delegate authority, and uninterested in institutional routine) but having a great capacity for work, a passion for control, and a fine political sense. Exploiting her social position and her post-Crimean War fame, she became an influential extra-parliamentary political force in the late 1850s and through most of the 1860s. Friends and family in high places—in the War Office especially—were flattered, shamed, or bullied into considering and occasionally adopting the rational but grandiose reform schemes she and her cadre of aspiring professional men and civil servants had devised and collected evidence to support.

As Smith properly insists, Florence Nightingale was a complicated person whose career is not easily weighed. He finds her choice of reform priorities fortunate and her ability to state succinctly and forcefully the assessments and solutions framed by more narrow-minded technical experts remarkable. She must be credited with hastening much-needed improvements in the army medical department and in hospital nursing. But it was power itself and not any one reform that seems to have motivated her, as she sometimes sacrificed a reform or her effectiveness as a reformer to score in petty intrigue or to boast of her influence. She could be ruthless with opponents—discrediting evidence, blocking publi-

cation, smearing reputations. With peers and superiors, she was both gracious and high-handed; with subordinates, authoritarian and vengeful.

Smith's book is carefully researched and stimulating. Its strength is its analysis of the Nightingale methods and relationships with politicians, civil servants, and other philanthropists. Although it is less convincing with regard to the difficult problems of her motives and alleged personality defect, it is a welcome reassessment of a great Victorian figure, one which should interest a large number of historical readers.

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P. J. WALLER. *Democracy and Sectarianism: A Political and Social History of Liverpool, 1868–1939*. Liverpool: Liverpool University Press. 1981. Pp. xix, 556. £24.50.

Liverpool is a sad city to visit. Lime Street station, bright and airy like a glass pavilion, is no longer crowded with passengers pushing on trains and off. Outside, the imposing statuary and tended lawns sweeping toward grand civic buildings convey the impression more of a stately home than of a bustling municipal center. Beyond lie acres of crumbling working-class terraces and quietly decaying middle-class houses. The business center near the docks is like the City of London on a Saturday. The docks themselves, endless stretches that once built and welcomed back much of the Atlantic's shipping, are empty. It is hard and haunting to remember that, until World War I, this was the great port-city where most Americans caught their first, exciting glimpse of the Old World.

It was, of course, also the port of entry for poor migrants from Ireland in search of wages to sustain themselves and the families they had left behind. That migration is P. J. Waller's unspoken starting point, for it gave rise to the embittered, often violent, and also colorful sectarian divisions that fragmented the working class of Liverpool and kept the Conservatives in power. The austere high-minded Nonconformist businessmen and conscientious reformers who led the Liberal party could not make enduring headway in this environment. The Labour party did not make much more headway in its attempt to overcome sectarian divisions with class solidarity. The Conservatives too had their problems. The prejudices that they harnessed were not monolithic but internally divisive. Crude assertion of these prejudices posed a cultural threat to the cooperation, upon which Conservative success depended, between the working and the middle and upper classes. The mobilization of support in the

Working Men's Conservative Association could have sharply reduced the ranks of men of property in the council chambers if the association had pressed for representation by men of its own class.

In painstaking, fine detail, alert to every nuance of strain and complication, Waller has put together an account of Liverpool's civic, parliamentary, and sectarian politics during seventy years of Conservative ascendancy. Though he gives the Liberals and Labour what due they earned, his story focuses naturally on the two Conservative bosses, Sir Arthur Forwood and then Alderman Salvidge, who forged and held together the sectarian and social alliance upon which the Conservative ascendancy rested.

The story often bogs down in unfocused detail, and the wit with which it is sprinkled is heavy. The main shortcoming of the book, however, is its failure to integrate the rise and fall of Liverpool's economy with its political and social story. This criticism is not just the too familiar request for more or a demand that the author do what he never intended to do. It is simply hard, if not impossible, to grasp the character of Liverpool's working class with its sectarian divisions and of the allied business interests without more than a sketchy impression of the flourishing, flaws, and fall of the unique economy of the city that gave or denied them their livelihood.

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JAMES JUPP. *The Radical Left in Britain, 1931-1941*. London: Frank Cass; distributed by Biblio Distribution Center, Totowa, N.J. 1982. Pp. viii, 261. \$30.00.

Given its current plight, it is not surprising that some scholars who sympathize with the general aims and values of the Labour party have been searching its history for origins and precedents in order to better understand present difficulties. In 1977 Ben Pimlott published *Labour and the Left in the 1930s* (strangely not cited in the volume reviewed here), in 1978 Lewis Minkin wrote *The Labour Party Conference*, and in 1979 Mark Jenkins wrote *Bevanism: Labour's High Tide*. Now we have James Jupp's *The Radical Left in Britain, 1931-1941*, but his conclusions that "the achievement of the Left was to enliven a cautious and conservative mass movement" (pp. 199-200), that "the Labour Left of the 1970s is in the same tradition as the Left of the 1930s" (p. vii), as well as the analyses which sustain these judgments, are likely to raise as many questions as they resolve.

Jupp's book is divided broadly into two sections. The first section attempts a chronological narration of the political history of the British left from the fall of the second Labour government in 1931 until the Nazi invasion of Russia in 1941; the second, which

covers ideological matters, attempts to establish a context for understanding the narrative of the initial section. Both halves contain insights into the history of the left in Britain, though the division between the two sections seems artificial and at times actively unhelpful.

The two most important organizations in Britain to the left of the Labour party during the 1930s were the Communist party and the Independent Labour party (ILP). Jupp rightly places them at the center of his narrative. Yet he makes no attempt to depict the leading personalities in either group, thus rendering the bitter factional disputes within and between them even more lifeless than necessary. The same is true, only more so, when it comes to the other sects and groupings on the left during the period. There was a bewildering array of them during the 1930s. Jupp bravely mentions them all, but often fails to introduce them or place them in perspective. They seem nearly indistinguishable, though this was not the author's intention. Worse, since Jupp has reserved his discussion of ideology for the second half of his book, we learn about the actions and decisions these groups took, but rarely about the ideological positions on which they were based. This interferes with his argument. For example, Jupp contends that the Communist party represented the greatest obstacle to unity on the left during the 1930s. But if one is to judge merely from resolutions passed at conferences (which is all Jupp's narrative offers), it would seem that the Labour party did most to oppose left-wing united action. Much more is needed here on the ideological conflict between communism and social democracy if one is to understand fully the Labour party's position.

The second half of the book is less confusing. In successive chapters Jupp treats the left's attitude toward "Revolution, Reform and Democracy," "The Role of the Labour Party," "The Future Society," and "War and Foreign Policy." He draws attention to an interesting and ironic evolution: during the 1930s the Communist party and the ILP nearly exchanged roles, the Communists moving from a revolutionary posture at the beginning of the decade to one that ostensibly embraced parliamentarism at the end, while the ILP traveled precisely the same route but in the opposite direction. Nevertheless, in this section too, one wishes for more. Despite its being devoted to the ideology of the left during the thirties, the major left-wing works of the period, for example, by John Strachey, Harold Laski, George Cole, or Palme Dutt, are not even mentioned, nor does Jupp attempt to explain the ideas of the important activists, of whom Stafford Cripps was preeminent.

One would also like to know more about the left during the 1930s than just what it did and thought.

There are demographic, economic, and social considerations as well. Where and why was radicalism strongest in Britain? Who was attracted to it and for what reasons? These are crucial matters that Jupp treats tangentially or not at all.

In conclusion, this is a book that examines the politics and ideology of the radical left in Britain during the 1930s, but not as thoroughly as one would like, and without evoking any feeling for the times. The issues, more even than the lessons of the thirties, remain for scholars and activists to ponder.

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ERIC RICHARDS. *A History of the Highland Clearances: Agrarian Transformation and the Evictions, 1746–1886*. London: Croom Helm; distributed by Biblio Distribution Center, Totowa, N.J. 1982. Pp. 532. \$28.50.

This book is the first of a two-volume study of the subject and comprises four parts: the wider framework, before the clearances, the pattern of pastoralism and clearance, and events—the focus narrowed. The second volume will consider interpretations of the clearances, a continuation of the present part 4, since the latter is based largely on contemporary sources. There will also be systematic analyses of popular resistance to the clearances, of the origins of the sheep farmers, and of the relationship between eviction and emigration.

While the highlands suffered from a particularly difficult set of environmental, economic, social, and institutional factors, they have been treated too long as a unique historical ghetto, and it is refreshing to see a solid attempt to link the clearances with much that was going on elsewhere. Furthermore, Eric Richards has successfully set them in full highland context by taking the trouble to determine the nature of the economy and society before the clearances.

It is now generally accepted that the failure of the second Jacobite rebellion in 1745 was not the root cause of the collapse of the clan system and of the mainly subsistence economy. Nevertheless, the highlands were only just beginning to enter the orbit of the world economy in the early eighteenth century: not for them the gradual transition of the long early modern period. By the end of the eighteenth century they were struggling to establish a specialized economy at the same time that population was increasing at a rate perhaps never before experienced in Western Europe.

Richards demonstrates the difficulties experienced in such a way that one is convinced that had there not been landlord-imposed solutions, drastic alternatives would have emerged, more widespread famine among them. Even well-meant and relatively

liberal attempts at reconstruction seem to have been doomed from the start. That is not to absolve the lairds of responsibility for much inhumanity and suffering, nor does Richards attempt to do this.

Despite the breadth of his approach, the author is guilty of an occasional lapse. Not enough is made of the fact that the highlands are a region of difficulty—barren, wet, exposed, and, above all, remote. True, this remoteness was broken down in the wake of the rebellion of 1745 by the building of roads, bridges, and quays, but many of the region's development problems are still based on its peripheral location relative to the main centers of European commerce. It is, therefore, a little naive to state (pp. 106–07): "In terms of geography, accessibility and climate, the Hebrides do not seem much less favoured than Lancashire as a zone for industrialization." With no merchant capital, no solid base of arable farming (as well as pasture), no great ports, no tradition of domestic production for sale, no cadre of petty entrepreneurs, no easy access to technological development, the Hebrideans had both hands tied behind their backs.

Nevertheless, one can recommend this study to those who wish to place the highlands more securely within the broad stream of European development.

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S. J. CONNOLLY. *Priests and People in Pre-Famine Ireland, 1780–1845*. New York: St. Martin's Press or Gill and Macmillan, Dublin. 1982. Pp. 338. \$30.00.

It is easy to write anecdotal histories of Irish religion—the country has had more than its fair share of crazed or eccentric clergy, despotic bishops, and obsessive-compulsive pilgrims. And it is equally easy to write the nation's religious history at a high theoretical level: the nation's sectarian cleavages, in particular, are the meat and drink of historical sociologists. The hard task is to merge the myriad of fascinating anecdotes with an interpretive framework that explains, rather than overwhelms, them. In this extraordinary book, S. J. Connolly of the New University of Ulster assays this hard task and succeeds triumphantly. This volume is the most original and important work on Irish religious history to appear in the past two decades. It sets a new and high standard of scholarship against which all subsequent Irish religious history, particularly that on the Roman Catholic church, will be measured.

The period upon which Connolly concentrates—roughly the seven or eight decades before the Great Famine—is at once pivotal in modern Irish history and difficult to work in: the story of the early years is enshrouded in the late penal-time in which relative-

ly few historical records were set down and even fewer preserved, and the integrity of the events of the 1830s and 40s is too often distorted by our foreknowledge of the impending famine. Connolly employs an exceptional range of primary material to illuminate this period and interprets them imaginatively, but with a scrupulous sense of their historicity.

Most previous work on the Catholic church in Ireland has concentrated either on the activities of bishops or upon dogmatic concerns. In contrast, Connolly's study deals with the everyday reality of Catholicism at the level of the parish, where priest and people meet. The picture he draws is subtle and textured, but in essence it is one of an ambivalent relationship between the Irish people and their priests. The Irish peasant and small farmer were willing to accept the clergy's lead on most religious matters, but on social questions and on certain religious issues the people and priests were in conflict. Only with great effort, and then with only gradual success, did the clergy succeed in gentling the people's behavior at wakes and fairs. And, significantly, in the case of certain religious practices (particularly patterns and pilgrimages), the local believers forced the clergy to continue these practices in many parishes long after the Irish bishops began to frown upon them. In this period the way that the people acted toward their church is so different from the situation in twentieth-century Ireland that one is, indeed, talking about quite another country.

This otherwise superb book illustrates one pitfall that other professional historians would do well to avoid. Its publication originates with a commercial press in Dublin, and it has not been copyedited at the level demanded by serious scholarship. The footnotes have been bunched together, mostly at the end of paragraphs, presumably to fit the economic limits of a trade book. In the long run, professional historians would be better off to shun the inducements of commercial publishing and to have their books published by firms that do them properly: by university and other scholarly presses.

DONALD H. AKENSON
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T. W. MOODY. *Davitt and Irish Revolution, 1846–82*. New York: Clarendon Press of Oxford University Press. 1981. Pp. xxiv, 674. \$54.00.

T. W. Moody has written a splendid biography of Michael Davitt. The book has been years in the making, but the wait was well worth it. The only other biography of Davitt was published in 1908 by Francis Sheehy Skeffington. Skeffington's biogra-

phy gave a sense of the man, but was not researched in depth. Moody's biography of Davitt reflects the careful work of a professional historian. It is well written and would be of interest to scholars as well as the general public.

Michael Davitt was one of the most interesting and important characters actively involved in the development of Irish nationalism in the last part of the nineteenth century. It is almost impossible to comprehend how Davitt was able to undergo the difficult experiences of the first thirty-two years of his life without being physically, mentally, or morally destroyed. When Michael was four years old his family was evicted from their small Irish farm for failure to pay the rent. Michael watched as the family placed their possessions on the road before their house was knocked down and set on fire. Forced to seek a living elsewhere, the family set out for England. When Michael was nine years old he became a factory worker. As a result of an industrial accident when he was eleven, he lost an arm. Michael became a Fenian at age nineteen. His revolutionary activity made him "uncomfortable about the presence of his parents and sisters in England" (p. 79), and he convinced them to emigrate to America where they could join his older sister and her husband who had left England several years before. One month after his family left England he was arrested and charged with having committed treason—a felony. He was found guilty, and at the age of twenty-four began a fifteen-year term of penal servitude. His life in prison was horrible.

Michael Davitt was not destroyed by his early experiences, but was actually strengthened by them. He was fortunate enough to have had a loving and supportive family and, while living in England, to have been part of an Irish community that provided similar love and support. Because of his industrial accident, he was not able to work. As a result, however, he was sent to school and received an education. His years in prison were difficult, but they were not destructive. He emerged from prison, after serving seven years, with his health intact and his commitment to Irish nationalism as firm as ever. In addition, his arrest and imprisonment had made him a public figure. Not only Irish nationalists but Irish tenant farmers and the Irish abroad could identify with Michael Davitt.

After leaving prison Davitt visited Ireland. There he heard of famine and evictions. Davitt concluded that Irish public opinion, united on the land question, could more realistically bring social revolution than a group of Fenians firing bullets at a military force. Davitt reasoned that if the Irish could obtain the transfer of the land from the landlord to the peasant the base on which English power rested in Ireland would be destroyed. He recognized the

importance of the intellectual currents and financial assistance that the Irish in America and England could give to the destruction of feudalism in Ireland. And finally, Davitt saw Charles Stewart Parnell as the leader who could bring the Irish together, cultivate the Irish abroad, and push the English to the negotiating table. Many of the events between 1877 and 1882 when Davitt and Parnell led the social revolution in Ireland were rooted in those early years of pain and pleasure which Davitt experienced.

The strength of this biography lies in Moody's ability to show how the experiences of Michael Davitt's early life molded his character and enabled him to join with Parnell to successfully carry out a revolution. Most of Moody's book deals with the period of the social revolution. Unfortunately, Moody does not go much beyond the period of the Kilmainham treaty. It is true that Davitt's role at stage center was over once Parnell made his treaty with William Ewart Gladstone, but Davitt's public life as a parliamentarian and world traveler, as well as his personal life, still remains to be detailed. In addition, Moody's explanation that Irish-American interest and involvement in Irish nationalism were due primarily to the fact that the Irish-American was neither welcomed nor accepted in America is too simple an explanation for such a complicated question.

But the fact of the matter is that Davitt's life has long deserved a careful study. Moody has now made it.

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RICHARD M. GOLDEN. *The Godly Rebellion: Parisian Curés and the Religious Fronde, 1652–62*. Chapel Hill: University of North Carolina Press. 1981. Pp. ix, 221. \$22.50.

The Fronde, a civil war—actually a series of conflicts—in France between 1648 and 1653, has been the subject of renewed attention during recent years. In addition to basic works by historians in France, historians writing in English (A. Lloyd Moote, J. H. Salmon, and others) have made important contributions. Although some points are still controversial, we know more about the complexities of the period than we did a few years ago.

Richard M. Golden, professor of history at Clemson University, has focused his attention on the rebellion of the Parisian curés. The decade from 1652 to 1662 is examined. What had been regarded as a rather minor episode is brought into sharper focus.

The curés, or parish priests, rallied to the support

of Cardinal de Retz, who was seeking to be installed as archbishop of Paris. Their motivation was complex. Not only did they see him as a defender of their right to administer affairs in their own parishes, against the meddling interference of Jesuits and regular clergy, but also they subscribed to the Gallican rejection of foreign control. To these causes, with which champions of the king might have agreed readily enough, the curés added a demand to be recognized as a corps separate from the upper clergy. This the crown found totally unacceptable.

It was in 1657 that Mazarin was finally able to deal energetically with the curés, closing their assembly and then consistently obstructing their renewed efforts to organize themselves as an independent corps. Significantly, when *cahiers* were again solicited in 1789, similar goals were expressed by some parish priests. As with so many issues of the Old Regime, this one had gone underground only to surface at the time of the Revolution.

Golden's book offers more than meets the eye. In addition to being an in-depth study of one aspect of the Fronde, it provides an insightful analysis of structural stresses and strains in the French church. Moreover, those abstractions mentioned in all the textbooks, Gallicanism and Jansenism, are illuminated by showing them in interaction with specific group interests. Students of the history of ideas will be rewarded by a clear analysis of the ecclesiology of Edmund Richer. Rather than treating such intellectual constructs in isolation. Golden relates them to the programs of the Parisian curés.

Richard Golden has written an intelligent book, an important book. Those wishing to understand the middle decades of the seventeenth century in France will not wish to ignore it.

DAVIS BITTON
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JOHN LOUGH. *The Philosophes and Post-Revolutionary France*. New York: Clarendon Press of Oxford University Press. 1982. Pp. 284. \$47.00.

THOMAS SCHLEICH. *Aufklärung und Revolution: Die Wirkungsgeschichte Gabriel Bonnot de Mablys in Frankreich, 1740–1914*. (Sprache und Geschichte, number 5.) Stuttgart: Klett-Cotta. 1981. Pp. 269. DM 88.

John Lough's aim is to discover how much of the practical program of the *philosophes*—a group for which he provides a precise but questionable definition—was effectively realized. His method is to extract from the writings of the group what they had to say on selected topics such as "trade and industry," "the problem of poverty," or "freedom of thought and of the press," and then to investigate to

what extent their goals were actually fulfilled during the Revolution or in the course of the nineteenth and twentieth centuries. The results are rarely surprising. In general, there is something mechanical about Lough's dogged inventorying of bits and pieces of Voltaire, Diderot, Helvétius, d'Holbach, Condorcet *et al.* in order to set them alongside actual legislation. It is not at all clear, to me at least, what historical or literary problem his inquiry was meant to illuminate. Perhaps the efficacy of ideas and the relation between social reflection and social action? If so, there is a troubling hiatus between Lough's exposition of the ideas of a small number of prominent eighteenth-century writers and his account of reforms that were actually carried out. The space of that hiatus seems to me to be where a truly historical investigation might be located.

As its title suggests, it is the space occupied by Thomas Schleich's book on Mably. Schleich has undertaken neither an immanent study of Mably's *oeuvre* nor an account of his influence on selected individual writers, but an investigation into the historical functioning of his moral and political writings. He traces their diffusion among successive generations of readers and the way they were used by different social interest groups to sustain and authorize the most diverse ideas and objectives. Patiently, and with great diligence, he unravels a complex, shifting, infinitely nuanced pattern of Mably interpretations: from the sixties and seventies of the eighteenth century, when the predominant reading was that of the reforming aristocratic and parliamentary circles with which the author himself was most closely connected; through the immediately prerevolutionary period, when his work was invoked by rival groups in support of divergent political theses; to the multifarious, often contradictory readings of competing factions during each phase of the Revolution. Subsequently, Mably's work was subjected to the increasingly radical readings of opposition groups under the Directory and the Empire; the deliberate neglect of the Restoration, which attempted to exclude him from the literary canon; and, finally, the nineteenth-century construction of a stereotype of Mably as precursor of socialism—a stereotype accepted by both left and right and confirmed at critical moments, such as 1848 or the Commune.

According to Schleich, this stereotype is still alive in Mably scholarship today. It has given rise, as in the past, to a politically motivated counter-image: Mably as a traditional moralist, steeped in the classical humanism of his Jesuit education and totally unsympathetic to the aims and aspirations of the Enlighteners. Contemporary Mably scholarship is thus a continuation of the very interpretative wars whose history Schleich's book relates. By shifting the focus of inquiry from a supposed immanent and

unitary meaning—of which each faction claims possession and which each uses to legitimize its own desires—to the diverse and divergent readings Mably's text has permitted, Schleich believes he has elevated his own study above the blind ideological conflicts other scholars have been caught up in. Not for the first time, the ironical vision is to facilitate a measure of emancipation from the prison of history.

It is our obstinate belief in transhistorical essences, in short, that prevents us from living our historical destinies consciously rather than unconsciously. The implication of Schleich's book is that there is no immanent meaning of a writer's total *oeuvre* or indeed of any part of it. Meaning arises only through the collaboration of the reader, who is not simply a passive receptacle for the author's seed but an active, creative partner, reading selectively, emphasizing some of a writer's works rather than others, and some aspects of these works rather than others. (An important part of Schleich's empirical research is devoted to determining which of Mably's works were most attended to by specific groups at specific times.)

The considerable merit of Schleich's displacement of emphasis from author to reader—a gesture that has become familiar to literary critics and seems inseparable from the general contemporary challenge to authority, origins, and the father—is that it forces us to look again at what we thought we saw well enough. Its inevitable consequence is the suggestion, at the end of the book, that far from the Enlightenment's having produced the Revolution, it may well have been the Revolution and its aftermath that produced our common conception of the Enlightenment. Not only are material facts or acts not produced by pure ideas; the reverse is more likely to be the case. Hence the need to reconsider critically what we thought we understood by "the Enlightenment" and to be prepared to discover something infinitely more complex and problematic.

Aufklärung und Revolution has both the old-fashioned positivist virtue of painstaking scholarship and the distinctively modern one of acute self-consciousness. It holds the inescapable historicity of texts in admirably steady view. Because it is so consistent, it highlights what for me is a troubling question. Schleich offers no immanent analysis of Mably's text, which is perceived only in its historical actualizations. Writer and reader alike are thus spared any confrontation with it. This may seem to be no great loss in the case of a text that has no literary value, as Mably's conservative critics in the nineteenth century alleged was the case. But an issue of principle is involved. The traditional literary critic tends to encounter a text not as an inert object of science but as a living power, a real "other." He is committed to the idea that any text is in principle

capable of being made to speak again and that it is his task to scrutinize even those commonly presumed to be dumb and inert for signs of life. Schleich would drive a deep wedge between the traditional literary method and the historical treatment of texts, with literary interpretation itself becoming the object of historical science or, at the very least, of an ironical historical vision. The social history of ideas, as Schleich and others propose to practice it, has no place for texts that are capable of responding and thus of provoking a present encounter; only texts that are presently inert and silent and that can be considered exclusively in their past activity fall within its purview. It would thus seem to be drawn to second-rate texts, whose present activity is extremely weak. Alternatively it may treat texts that are capable of evoking a response as if they were not, turning a deaf ear to their blandishments. Not for nothing, apparently, did Furet change the original title of his famous project from *Littérature et société au XVIII^e siècle* to *Livre et société au XVIII^e siècle*.

In the deliberate choice of texts that offer the minimum of resistance to the historian's disposing of them "scientifically," it is difficult to avoid recognizing something very similar to the illusory sense of superiority, of which Nietzsche accused another generation of historians, no less active and successful than the present one. The resolutely antielitist commitment to the (preferably second-rate) text-as-object may also bring with it a whiff of philistinism. Although it is a minor point, it is disturbing that in his concluding reflections Schleich follows Darnton in classing Chamfort with Cailhava, Cubières, Garat, Roucher, Suard, and Target as a writer of mediocre talent and no real accomplishment, interested primarily in "making it" in the literary world of the High Enlightenment. I cannot speak about the others in this list, but I find it hard to believe that anyone who has read Chamfort could accept this summary judgment of a writer Schopenhauer and Nietzsche placed on a par with Montaigne, La Rochefoucauld, and Lichtenberg. No doubt Schleich did not think it was necessary to read Chamfort, but there is no evidence that he was much affected by his reading of Mably, who may indeed be as dull as the nineteenth-century conservatives said he was. Now what kind of history of ideas or of literature will be produced by those who elect not to encounter the ideas and texts of the past as living forces in the present? What view of ideas and of literature, what respect for them can they have or evoke in others? What has happened to Michelet's democratic yet pious dream of resuscitating the past and "making history's silences speak"? On the evidence of its productivity and large work force, the business of Enlightenment is prospering as never before, but how will the accountants judge it when they come to examine the books?

A final ungracious but necessary observation. The editing of this fine book has been unbelievably sloppy. There are misprints or misspellings everywhere ("Didérot" consistently for "Diderot," "Karazime" for "Karamzine," and "Raymond de Cinq Mars" for—presumably—"Rémond de Saint Mard"). Some of the bibliographical references are a joke.

LIONEL GOSSMAN
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MICHAEL L. KENNEDY. *The Jacobin Clubs in the French Revolution: The First Years*. Princeton: Princeton University Press. 1982. Pp. xii, 381. \$27.00.

This volume is the first of a projected three-volume synthesis on the network of Jacobin clubs in the French Revolution. Michael L. Kennedy is well qualified to carry out this important project. Having published a fine study of the powerful Marseilles club in 1973, he has since been combing local archives and newspapers for this new study to an extent that far exceeds the research in earlier syntheses by Crane Brinton and Louis de Cardenal over fifty years ago. The current volume will be followed by one on the middle years and then by one on the Terror. Each will be organized topically and, judging by the present volume, will reflect a prodigious knowledge of the sources and the subject. I believe that the second of the three will offer the most interest in the long run, for it will deal with a relatively neglected phase with numerous interpretive problems—notably the character of the radical turn in the Revolution's politics. The present volume, however, is obviously the indispensable groundwork for the next, as well as an important contribution in its own right.

Kennedy's research is difficult but rewarding, allowing him to see the network as a whole. He is in a position to generalize in a field in which the local circumstances of individual case studies often inhibit scholars from generalizing. He does not necessarily ask or answer all the pertinent questions, and on occasion spends too much time on matters long since settled, but on the whole this is a fresh portrait that will stand as a reliable reference point in the field.

Some of the chapters are superb, especially those that involve a systematic canvass of the clubs. These include a chapter on the press and the clubs (whose conclusions are surprising) and one on the Feuilleant-Jacobin schism (which traces a crucial struggle for the movement's soul). Empirical data on the founding and geographical distribution of clubs, discussed in the text and reproduced in appendixes, are invaluable. On the other hand, a couple of

chapters, those on economic issues and on egalitarian thought, are scattershot and leave an inconclusive impression.

No more than any mortal, Kennedy cannot resolve all the problems inherent in moving from the particular to the general and back again. But in his hands the widely dispersed and fragmented records of the clubs are impressively assembled. In an unprecedented fashion he can then trace the progression of a particular *démarche* from one club to a succession of others, providing a vivid sense of just how these eminently local organizations also constituted a potentially powerful network even before the establishment of a dominant power center in Paris. The Revolution might well have been "a magnificent irrelevance" (in Richard Cobb's phrase) for many French people. But Kennedy's project will reflect the other side of this history: how widely the Revolution took root in urban civilization across France. At the risk of sounding ritualistic, let me conclude by saying that volume 2 is eagerly awaited.

ISSER WOLOCH
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DAVID COHEN. *La promotion des Juifs en France à l'époque du Second Empire, 1852-1870*. Volume 1, *Pouvoir et minorité*; volume 2, *La promotion et l'intégration*. (Études Historiques, number 5.) Aix-en-Provence: Publications Université de Provence; distributed by Jeanne Lafitte, Marseille. 1980. Pp. xix, 329; 330-869.

General wisdom dictates against publishing a thesis without major revision. David Cohen's book reminds us once again why. Exploring the Jewish community of France fifty years after the Revolution, the two-volume study examines public figures from the emperor to municipal councilors as they dealt with the Jewish minority, focuses on the "tendency" toward integration and expressions of antijudaism, and concludes with a multi-dimensional portrait of the French Jews under Napoleon III.

The work is preceded by an introductory chapter providing the historical background since emancipation (1790-91) and a chapter on the demography of the Jewish population during the Second Empire. Throughout, Cohen neglects to deal with the internal life of the Jewish communities—his orientation, as he acknowledges, is *histoire externe*.

Cohen's assumptions are optimistic, his presentation rosy. Successful legal integration by 1846 and economic integration completed in most urban areas by the 1870s permitted a rapid advancement by Jews, especially in banking and the press. The absence of Jews in the highest political and military echelons did indicate a certain *prévention* on the part of the Second Empire's administrators, and social integration, existing among the bourgeoisie, collid-

ed at times with antijudaism of both the left and the right. In general, however, the Second Empire achieved a state of harmonious equilibrium with its religious minority, which the emperor demonstrated with *attention* and *délicatesse* at his Vichy cures. The regime of Napoleon III, Cohen concludes, would not have tolerated a Dreyfus affair.

For any student of French Jewish history, these 863 pages provide a rich mine of information and an impressive encyclopedic breadth. Unfortunately—and here the characteristics of a dissertation emerge—they also provide too many quotations unrelieved by integrating analysis and a frustrating unevenness of interpretation and style. The passages depicting the emperor and empress and their *ouverture d'esprit* partake of a nineteenth-century *hommage*; on the other hand, Cohen's chapter on the *mentalité* of the French Jews is an informative, panoramic view of the community as it circumscribed, educated, buried, and rarely intermarried.

The period as a whole is evoked in a Whig interpretative style; the progressive stages of economic, social, and legal amelioration form the backdrop against which interactions, developments, issues, and individuals are portrayed and evaluated. The hint of an apologia, moreover, highlights the need for continued, serious collaboration between French historians and historians of the Jews of France. Nonetheless, Cohen's book is useful, the illustrations enjoyable. An index, essential for a work of such magnitude, ought to have been compiled.

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BONNIE G. SMITH. *Ladies of the Leisure Class: The Bourgeoises of Northern France in the Nineteenth Century*. Princeton: Princeton University Press. 1981. Pp. xii, 295. Cloth \$22.95, paper \$9.95.

Recent work on the lives of middle-class women has stressed the separation of male and female spheres and the ways in which women created an interior world of their own within the home. Much of that literature, however, has belabored the stifling nature of women's sphere. In so doing, it has failed to view middle-class women in their relation to industrial capitalism, market values, and the democratic individualism, which were such central components of nineteenth-century life. Bonnie G. Smith's *Ladies of the Leisure Class* successfully corrects these omissions by showing how the nineteenth-century bourgeois Frenchwoman's domestic world and domestic values provided alternatives to the intensely competitive, materialistic, industrial society that developed around her.

Smith focuses on the women of the textile cities of

the *département* of the Nord, especially Lille, Roubaix, and Douai. Drawing heavily on anthropological and psychological theory, she analyzes family letters, diaries, novels, the papers of female-dominated institutions, and, perhaps most interestingly, the symbols and rituals of women's daily lives.

For a brief period in the late eighteenth and early nineteenth centuries, bourgeois women participated in the management of the large, family-based textile firms of the Nord. They kept the account books, bargained with clients, passed judgment on the quality of goods, and occasionally appeared in court to defend actions they took against workers.

Their business activity in this arena, however, was relatively short-lived. Generations coming to maturity later in the century cared little for business. As the textile industry grew and the division of labor within it became more complex, home became more clearly separated from the world of work. The bourgeois woman of the second half of the century confined herself to the domestic sphere, where she developed her own "rhetoric of reproduction."

As reproducers of their class and as bestowers of order upon the home, these women were not merely reflections of their husbands' wealth and status; they built a culture distinctly their own. Smith's keen eye for domestic symbolism seeks out the ways in which women's space and women's consumption reinforced their mothering roles and came to represent the denial, rather than the assertion, of industrial values. The velvet cushions and lacy shawls adorning their homes were the very opposite of the rational, calculating spirit of the market, as were the intricate fashions in which they increasingly indulged themselves toward the end of the century.

Religious devotion was another element of the bourgeois woman's domestic system, offering an acknowledgment of her inferior place in the world and a means of accepting it. Moreover, in her religious activities, she emerged as the defender of the traditional order against republicanism, liberalism, and the chaos of the individualistic modern world.

In fine chapters on charity and education, Smith shows how the *dames patronesses* of the Nord organized to offer assistance to poverty-stricken (but not unwed) mothers, as well as to establish kindergartens, *crèches*, recreational groups, *ouvroirs*, and schools. These women were not mere "ladies bountiful," filling up their spare time, escaping from the claustrophobic atmosphere of their homes, but women who sought to impose their domestic, heavenly order on the outside world. All of their charitable activities reflected a "commitment to the family . . . centering on woman and her reproductive activities" (p. 144).

Nor were these activities without their political implications. As women succeeded in capturing for

themselves and the church the allegiance of working-class families, they threatened Republican hegemony under the Third Republic. Consequently, in the 1880s public support to women's charitable organizations was withdrawn, and the state took over welfare programs for the poor and working classes. A similar fate befell women's educational institutions. Long devoted to the principle that ignorance and innocence were signs of virtue, they could not survive for long once Republican reformers finally began to question the ability of poorly educated women to train solid French citizens.

Ladies of the Leisure Class is a meticulously researched and closely argued work. Smith is to be commended for her forthright, interdisciplinary approach, which provides the basis for a rich and refreshing interpretation of the lives of middle-class women. She goes well beyond the stereotypical picture of the idle, leisured *bourgeoise* by now so familiar to students of women's history.

LAURA LEVINE FRADER
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CHARLES SOWERWINE. *Sisters or Citizens? Women and Socialism in France since 1876*. New York: Cambridge University Press. 1982. Pp. xx, 248. \$24.95.

French women of the *belle époque* and after generally avoided the current of feminism then sweeping other industrial countries. Not only bourgeois women but also socialists refused to develop the kind of sturdy, woman-oriented groups for which their German counterparts gained such renown. The German model looms large in Charles Sowerwine's history of French women socialists. As late as the 1970s he finds the latter by comparison far more interested in the advancement of their class than in the well-being of their sex. And as a result of neglecting women's issues, the party had only a minuscule female membership until fairly recently.

The culprits in this sorry state of affairs, according to Sowerwine, were that handful of women who did adhere to socialism, and in particular Louise Saumoneau. Motivated by her antagonism toward bourgeois feminists, Saumoneau determined that every women's group she participated in should draw its membership exclusively from party members. Within the party itself, she wanted no special women's sections for fear that they might spin off into feminism or detract from the regular organization of the movement. Dominating the various groups formed in Paris at the turn of the century by her strength of character and will, Saumoneau was even more influential in the interwar period because of her firm internationalism during the war. Her legacy of no recruitment of women outside the party and no espousal of sisterhood within prevented women from finding in socialism the female

analogue to *fraternité* so attractive to the male proletariat.

In Sowerwine's overview socialist women from Aline Valette to Madeleine Pelletier and Suzanne Buisson constituted, along with Saumoneau and others, a cluster of antiheroines who failed women of the French working class. Indeed, "failure" and its cognates appear dozens of times in this book alongside the names of the women socialists. Theoretically confused, inept at seizing the organizational main chance, they were, to his way of thinking, a pathetically myopic lot in comparison with the Germans. Perhaps they were, but there may be another way to tell the story. Women like Pelletier, Valette, or Saumoneau need to be considered at least to some extent on their own terms, with more attention paid to the general political orientation of French working women, their culture and work life, and their indigenous *modus operandi*. From an internalist perspective their ideas and programs might deserve higher grades. But having a fixed idea of what socialist women *should* have done, rarely can Sowerwine give a "surprisingly good" (p. 156) to a speech. Yet despite his stern schoolmasterish stance, one cannot help but be pleased with this survey of an important topic and its suggested answer to the riddle of French women's avoidance of socialist politics. Not being citizens, they found political debate and maneuvering irrelevant. Offering nothing in the way of gender solidarity either, the party in the long run offered them nothing at all.

BONNIE G. SMITH
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GÉRALDI LEROY. *Péguy entre l'ordre et la révolution*. Paris: Presses de la Fondation Nationale des Sciences Politiques. 1981. Pp. 294.

This small volume is an attempt to rescue Charles Péguy from his confinement in the pantheon of French literary culture. A sense of the unity of Péguy's thought has suffered from the very quality of his literary expression, leaving us with an overabundance of apt quotations, surely the most famous of which is "Tout commence en mystique et tout finit en politique." Men of many shades of political opinion have tried to borrow some of his mystique; both Vichy and the Resistance claimed the author of *Jeanne d'Arc*.

Géraldi Leroy reconstructs the social context of Péguy's thought, paying proper but brief attention to his subject's biography and revealing in the process the republican, secular, and rationalist roots of Péguy's early interest in the social question. Like so many of the Dreyfusards, Péguy moved toward socialism from disgust with bourgeois politics and

humanitarian concern for the poor rather than from a sympathy for the working class. Despite his religious upbringing, he did not gravitate toward "Christian socialism"; it was, in Leroy's estimation, too obvious that the church was fully allied with capital. Leroy finds Péguy's earliest concern with the social question lacking in any understanding of the factory worker. His sympathies remained rooted in his artisanal past even as his politics became militant. Intellectually, Péguy's socialism was as rooted in neo-Kantian imperatives and the influences of friends (Jaurès, Herr, Andler, Daniel Halévy, and Blum) as it was in Marx or Guesde.

Fundamentally pessimistic, only tangentially related to Marxism (at a time when Marxism was gaining influence in France), profoundly individualistic to a point of anarchism, and moralistic in judging the political tactics of socialist politicians, Péguy's critical stance made him a difficult, perhaps impossible, ally for socialists. For a decade before the First World War, German power so alarmed him that he must have sounded to his comrades rather more like an integral nationalist than a socialist. He had no sympathy for violence in the streets or for the authoritarianism of Barrès and Maurras, but he was at bottom a French patriot. Péguy's passivity in the face of labor unrest on the eve of the First World War is for Leroy the real test of his socialist commitment, a test he failed. According to Leroy's summation, Péguy never understood exploitation as an integral part of the capitalist system, never understood revolution, accepted the civilizing mission of colonialism, was patriotic to his fingertips, and ultimately remained a petit bourgeois in values and culture.

Leroy's final summation, or rather reduction, is the flaw in this useful study. He finds Péguy wanting, but his intellectual biography has shown us one of the most illustrious of the generation of Dreyfusard socialists, men of contradictions and conflicts, whose socialism was an admixture of values, politics, and good will.

Péguy's joining of socialism and nationalism was a fascinating and not at all unique phenomenon. Leroy's postulate that Jaurès is the last thinker of the nineteenth century and Péguy the first of the twentieth deserves more of Leroy's attention—and ours.

MYRNA CHASE
Baruch College

DOUGLAS PORCH. *The March to the Marne: The French Army, 1871–1914*. New York: Cambridge University Press. 1981. Pp. viii, 294. \$44.50.

In *The March to the Marne*, an iconoclastic, provocative history of the French army from 1871 to 1914, Douglas Porch has posed a series of major questions

not only about civil-military relations but also about military organization and doctrine. Was the officer corps dominated by aristocrats whose politics were militantly antirepublican? What was the purpose of Radical-inspired reforms after 1899? Did the nationalist revival after 1911 restore the professional army? In 1914 were the commitment to the tactical offensive and the lack of heavy artillery the result of coherent policy and doctrine? Was the Dreyfus affair simply a temporary aberration in civil-military relations from which both nation and army soon recovered?

Porch, Lecturer in History at University College of Wales, answers these questions from the perspective of the French army, particularly of the officer corps. Moreover, his underlying premise is that the accepted model of French military history has been created by a group of historians whom he labels the "nation-in-arms" school. These writers, he maintains, have uniformly misinterpreted the era from 1871 to 1914 as "a battle between the professional army and the 'nation-in-arms'" (pp. vii–viii). They have set forth the mistaken view that a reactionary officer corps, ever resistant to democratic reform, not only produced the Dreyfus affair but also—by virtue of its devotion to the professional army, its scorn for the reserves, and its commitment to the offensive—led France to near disaster in 1914.

But, to Porch, the French army has always been the victim of the vagaries of French politics. The composition of the officer corps was middle class rather than aristocratic, and French soldiers have been wedded to order and political neutrality rather than to any particular regime. The Dreyfus affair, neither an antirepublican conspiracy nor an antisemitic plot, did not originate in the socio-religious beliefs of the officer corps. Rather it was "sparked by an institution founded and nurtured by republican reformers and regarded as distinctly middle class in its composition and outlook—the general staff" (p. 57). "The refusal to review Dreyfus' conviction and the false evidence manufactured to throw sand in the eyes of justice did not reflect an officer corps seeking any excuse to defy the republic, but rather an incestuous and exclusive general staff" (p. 58).

Porch locates few heroes in the ranks of the Third Republic's politicians. Almost his only kind words are for the moderate Republicans of the Gambetta era who consciously tried to win the army over to the republic by supporting its professional interests. By contrast he excoriates the Radicals, who came to power with the Dreyfus affair and whose abiding purpose, under the sneaky cloak of "republicanization," was to politicize the officer corps by making promotion dependent not on merit but on political favoritism. "Radicals were not reformers, they were destroyers" (p. 103), and their so-called reforms

"were a deliberate attempt . . . to lower the social and intellectual level of the officer corps" (p. 83). Aiming at publicity rather than results, they "pursued a petty vendetta in which the weapons were trivial insults and demanding decrees . . . [intended] to make service to the Republic a purgatory, forcing officers to conform or resign" (p. 56).

Thus the shortcomings of the army in 1914 were the direct consequence of the bruises and insults that the politicians had inflicted on the soldiers. Radical policy undermined morale and efficiency, leading the officer corps to feel disowned by society and to seek refuge in careerism, administration, and bureaucracy. The lack of heavy artillery in 1914 resulted from the army's having been so victimized by the politicians that it lacked the intellectual capacity to formulate a doctrine. The commitment to the offensive was a confession of weakness and not an assertion of strength: "it sprang from the army's very lack of confidence, its poor organization and material weakness" (p. 214). Moreover, the much-heralded nationalist revival after 1911 was both too little and too late: "far from restoring the army's confidence, [it] appeared only to emphasize how far army morale had slipped" (p. 189). The Dreyfus case had produced "a wall of distrust" between the army and the republic, a barrier that even the *union sacrée* of 1914 could not breach. The officer corps was unforgiving. Porch concludes: "When defeat appeared certain in 1940, a generation of soldiers who had suffered arrogant politicians since 1900 did not pass up the opportunity to pull the rug from under its tormentors" (p. 254).

There is much to commend in this interpretation, sensitive as it is to the laments of the soldiers. The discussion of the debate over heavy artillery clearly demonstrates the intellectual paralysis of the high command; the critique of the structural weaknesses of the war ministry and the general staff is equally convincing; and there is much new material, notably on the extent of indiscipline and on the phenomenon of unionization, that does not appear in other studies of the French army. Porch's chapter on the colonial army—admittedly a paean of praise that presents it as a "shaft of light" precisely because it escaped the fate of the metropolitan army—is a brilliant appreciation of its leaders, its ideology, its sense of mission, and, above all, of the dynamics of French colonialism.

But there are grave flaws. Porch's eagerness to dismiss the Dreyfus affair as simply "a blunder produced by a blinkered bureaucracy" (p. 60) and his obvious contempt for French politics, especially Radical politics, lead him to minimize the other forces in French society that affected the position of the army. He presents no evidence to back up his assertion about 1940; it stands as a last petulant outburst against the turn-of-the-century politicians

whom he detests so much. Evidence is used selectively. The chapter on the three-year law, which accepts at face value the soldiers' case for extending the length of military service, focuses not so much on that actual debate in 1913 but on the argument that the reserves were weak and the number of NCOs insufficient. Similarly the discussion of the nationalist revival emphasizes those things, which, according to French officers, it did not accomplish. Indeed, Porch's tendency to write from within the army, to quote the disaffected officers, and to echo their distrust of politics makes much of his book read like a soldier's brief against the republic. In the process he overlooks the phenomenon of military pessimism: the tendency of most soldiers at most times to view political and military circumstance in the light of worst possible contingencies. Porch also attempts to be much too clever by portraying himself as the first historian to challenge the allegedly monolithic "nation-in-arms" school. He has, in reality, set up a straw man. Monteilhet and Michon, French historians of the left who write in the 1930s are, to be sure, fair game. But both post-1945 French and American historians—Pierre de la Gorce, Raoul Girardet, David Ralston (whose interpretation of Joffre is subjected to an unfair, out-of-context criticism) and Philip Bankwitz (whose sympathetic appreciation of Weygand is strangely missing from an otherwise impressive bibliography)—have produced far more balanced and varied interpretations than Porch is willing to admit.

RICHARD D. CHALLENGER
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HAIM AVNI. *Spain, the Jews, and Franco*. Translated by EMANUEL SHIMONI. Philadelphia: Jewish Publication Society of America. 1982. Pp. xi, 268. \$19.95.

Spanish government assistance to European Jews is a minor issue of the Hitlerian Holocaust that has attracted some interest in recent years, producing two modest books (little more than booklets), several articles, and a few propaganda declarations by the former Spanish regime. Students of Spanish politics and diplomacy, as well as of the Holocaust in general, have hoped for a more thorough study that would clarify the matter in full detail. Although Haim Avni's new work is not the complete and definitive treatment that might have been desired, it accomplishes much more than any previous statement and answers most, though not all, of the questions that have arisen about Spanish policy.

The scope of the book is greater than that of Spain and the Holocaust alone, for it endeavors to review Spanish policy toward Jews and the general history of Jews in Spain during the nineteenth and twentieth centuries as well. Less than one-fourth of

the book is, however, devoted to this broader sketch of Spanish-Jewish affairs so as to concentrate the great bulk of the study on Jews in German-occupied Europe who in one way or another came within Spanish purview.

The author approaches the subject from the viewpoint of the problems of Jews rather than that of Spanish policy. He has informed himself sufficiently about the latter to present a generally accurate account of the framework of Spanish history and institutions, though his historical treatment is occasionally incorrect in detail and more frequently somewhat misleading in nuance. Franco appears in the title of this book, but he is not featured to any extent in the account itself. True to his basic style of government, the Caudillo rarely intervened in the details or execution of Spanish policy regarding Jews, so that the chief actors on the Spanish side are normally the foreign minister and high-ranking diplomatic and consular personnel.

Concerning the extent of genuine Spanish assistance to beleaguered Jewry, Avni presents what would seem to be an accurately ambiguous response. Spanish policy was normally correct and in itself nondiscriminatory; contrary to the image sometimes generated by the Spanish regime in later years, Madrid did not very consistently exercise energetic initiative on behalf of Sephardic Jews or any others. The most remarkable accomplishments in Hungary and the Balkans were due to the local and personal initiatives of the consul general in Athens, Sebastián Romero Radigales, and the chargé in Budapest, Angel Sanz Briz, who are the real heroes of this encounter. With regard to numbers, Avni estimates that during the first half of the war, when Spain opened its transportation facilities to all correctly documented Jews who appeared at its borders but exercised no initiatives of its own, thirty thousand Jews gained transit through Spain to havens elsewhere. During the second half of the war, Spanish officials acted to save approximately eleven thousand five hundred Jews. Of these seventy-five hundred were refugees who reached Spain under diverse national programs; 3,235 were Balkan Sephardic and Hungarian Jews, who received diplomatic protection; and about eight hundred were Spanish nationals repatriated through Spanish initiative. Avni contends, perhaps correctly, that the Spanish regime might have exerted itself even more had the Western Allies placed greater pressure on it in 1943–44 to use its good offices in German-occupied areas.

The book altogether fails, however, to provide a thorough examination of political and cultural attitudes toward Jews in twentieth-century Spain, a task that would have extended the study much further. Thus there is no mention of the basically positive attitude toward Sephardics of some of the military

leaders of early twentieth-century Spanish nationalism in the Moroccan protectorate, or of the enthusiasm for Sephardic culture manifested by Ernesto Giménez Caballero, who introduced fascist ideology into Spain. The highly divergent attitudes toward Jews among Falangists and within the early Franco regime are passed over much too briefly to provide clear treatment.

All books should be noted more for their achievements, however, than their shortcomings. Avni's accomplishment is to provide us with the first full and reliable scholarly account of Spanish policy and initiatives toward Jews during the Second World War, and it is a very worthwhile contribution.

STANLEY G. PAYNE
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GEORGE KUBLER. *Building the Escorial*. Princeton: Princeton University Press. 1982. Pp. xvi, 185. \$40.00.

Few monuments on earth so impress the viewer or grip the imagination as does the Escorial, built by Philip II of Spain some thirty miles from Madrid. Alone, it stands as a work of art; more, it reveals much about its builder and his era. In his classic *Rise of the Spanish Empire*, volume 4 (1934), Roger B. Merriman claims "it sums up the Spanish Empire in the period of Philip II more than any book can possibly hope to do" (p. 43).

In this superb, illustrated study, richly produced by Princeton University Press, George Kubler tells the story of the construction of the Escorial and considers the aesthetic principles that shaped it. He draws heavily on archival sources and contemporary evidence to trace the building from its conception to its completion.

Kubler begins with a survey of four centuries of critical commentary extolling or denigrating the great monastery-palace. He then focuses on Philip—his knowledge of architecture and his intention to build a suitable resting place for his parents, a retreat for himself, and a residence for the favored Jeronymite order of friars.

In a masterful chapter on "The Escorial: Design and Antecedents," Kubler argues that the final result, in its massive simplicity, owes much to Italian influences, especially that of Francesco Paciotto, with his interest in Vitruvius and experience in military architecture. He pays close attention to the responsible architects, Juan Bautista de Toledo and, above all, Juan de Herrera. The former was a professional, who had worked with Michelangelo. Personal problems rendered him ineffective, and only the foundations had taken shape when he died in 1567. Herrera's background was that of a human-

istically educated courtier, an amateur at architecture, yet he guided the construction to its completion.

Building the Escorial involved the friars who oversaw the work, the architect, contractors, masters, foremen, and hundreds of artisans and laborers. Detailed tables of expenses and contracts supplement the almost stone-by-stone (from quarry to site) story of the fabric, which Kubler illuminates with comment on architectural and aesthetic concerns. He neglects neither the grounds, gardens, and outbuildings, nor events after 1600, which included the building of the present royal crypt (1617–54) and the Escorial's survival of fires, wars, and a plague of termites.

In a convincing epilogue, Kubler disagrees with René Taylor's contention that the Escorial is a monument to the king's and Herrera's interests in astrology and the occult (see *Essays in the History of Architecture Presented to Rudolph Wittkower* [1967]). Kubler sides with the Escorial's first historian, Fray José de Sigüenza, and finds the Escorial's prevailing aesthetic to be Augustinian.

A comparison deserves to be made between this fine book and Jonathan Brown and J. H. Elliott, *A Palace for a King* (1980). The latter is a groundbreaking collaboration that brilliantly places the building and decoration of Philip IV's Palace of the Buen Retiro in the full context of the statecraft of Olivares. What Kubler says of Philip II and the Spanish empire is skimpy and somewhat out-of-date. His is architectural history at its best, but it remains within traditional confines.

PETER PIERSON
University of Santa Clara

A. C. DE C. M. SAUNDERS. *A Social History of Black Slaves and Freedmen in Portugal, 1441–1555*. (Cambridge Iberian and Latin American Studies.) New York: Cambridge University Press. 1982. Pp. xviii, 283. \$49.50.

Thanks largely to the influence of the so-called *Annales* school, the study of Portuguese social and economic history has enjoyed a period of revitalization since the late 1950s. Studies by Frédéric Mauro, Vitorino Magalhães Godinho, Albert Silbert, António de Oliveira, and others have added much to our knowledge of the socioeconomic regime in early modern Portugal. Nonetheless, this area of research remains a vineyard tended by too few laborers. It is therefore especially pleasing to welcome the appearance of A. C. de C. M. Saunders's fine social history of blacks and freedmen in Renaissance Portugal.

The period examined extends from the first Portuguese slave raids in 1441 to the publication of Fernão de Oliveira's *Arte da Guerra no Mar* (1555), a

seminal and thoroughgoing attack on the slave trade. The latter date also roughly coincides with the rapidly expanding traffic in black slaves to the Americas. Although slaves in Spanish America and colonial Brazil usually came to be regarded as little more than human machinery, their status in Portugal was, as the author clearly shows, that of inferior retainers whose social position was similar to that of dependent children.

Unlike their counterparts in colonial Brazil, Portuguese slaves were mainly employed to supplement existing labor. No occupation was exclusively associated with blacks, and there is little evidence that slaves were employed in large-scale plantation agriculture. The author calculates that blacks comprised no more than 3 percent of the national population, though local concentrations, particularly those in Lisbon, Évora, and the Algarve, may have reached 10 percent. Extensive consultation of notarial records confirms that the bulk of the black population lived in Lisbon and regions south of the Tagus.

Perhaps the most original contribution of the study is that portion dealing with the occupations and daily life of blacks. The author provides much valuable detail on the range of occupations open to slaves and freedmen and finds, for example, that blacks distinguished themselves in the olive-oil industry and as innkeepers, an occupation especially suited to free black women. Useful detail on diet, dress, and other aspects of daily life is provided, as are insights into the lives of lower-class whites, who were often on friendly terms with blacks. The question of race relations is further illuminated by Saunders's judicious reading of contemporary literature, particularly stage plays.

Appreciation of this well-written and exhaustively researched monograph is somewhat tempered by its spare conclusion. Fuller discussion of Portugal's central role in the spread of black slavery to the Americas and more developed comparisons between its Old and New World variants would have better tied the study to slavery's widening colonial context. This is, however, only minor criticism of a study that establishes its author as a talented worker in the Portuguese vineyard.

CARL A. HANSON
University of New Mexico

JOHN MICHAEL MONTIAS. *Artists and Artisans in Delft: A Socio-Economic Study of the Seventeenth Century*. Princeton: Princeton University Press. 1982. Pp. xvi, 424.

Far too little is known about the social and economic situation of Dutch artists in the seventeenth century. Most of what has been written on the subject has

been based on a combination of anecdotal evidence and haphazardly collected documentary materials, heavily biased toward those painters who arouse most interest today. Thus the appearance of a book relying on thorough and systematic archival research and concentrating on just this problem is most welcome, even though the research is restricted to Delft, which may very well not be typical of Dutch towns. John Michael Montias has found his evidence principally in the archives of the guild of St. Luke and in the town's notarial archives—a rich if intractable source of evidence for a great many aspects of social and economic life. Besides the painters, he has studied the other crafts belonging to the guild, in particular the potters who made Delft faience, in order to compare, albeit in a limited way, the painters with other craftsmen of the time.

His research will make it necessary to adjust somewhat our ideas about both the Dutch artist and the art market, but in neither case perhaps as much as the author would seem to believe. Montias presents us with a wide range of evidence, much of it illuminating, some of it less so. Probably the two most important points he has to make are that the economic and social position of the artist was distinctly higher than has previously been thought and that the essential market for Dutch art was more exalted socially than has usually been supposed. On the first point, he argues that the social origins of painters were solidly middle class, that their relatively expensive training was beyond the means of lower-class parents, and that their incomes were significantly higher than those of most skilled workers. What his evidence shows, however, is that in the main painters were the children of skilled craftsmen—which is the accepted view—and that some painters made a very good living while others did not. The information he presents does not really allow any very firm conclusions. Perhaps the point really is not that painters as a whole were particularly well off but that the rewards of success could be considerably greater than in most crafts. On the question of who bought the works of the Delft master painters (as opposed to hack work produced crudely and cheaply for a popular market), his answer is that it was the wealthiest third of the population and not a broader lower-middle-class group. The evidence to support this conclusion is not very strong; but in any case it is not so far from the accepted view, which would look to, very roughly, the richest half of the population to find the market for Dutch art.

Nevertheless, this is a stimulating book, packed with information and sprinkled with interesting ideas and speculation.

J. L. PRICE
University of Hull

LASSE CORNELL. *Sundsvallsdistriktets sågverksarbetare, 1860–1890: Arbete, levnadsförhållanden, rekrytering* [The Sawmill Workers of the Sundsvall District, 1860–1890: Work, Living Conditions, Recruitment]. Summary in English. (Meddelanden Från Ekonomisk-Historiska Institutionen vid Göteborgs Universitet, number 49.) Göteborg: Göteborgs Universitet. 1982. Pp. 388.

This doctoral dissertation by Lasse Cornell is part of a larger study, "The Standard of Living in Sweden, 1800–1970." It attempts to describe the living conditions of the sawmill workers in the Sundsvall area between 1860 and 1890 largely on the basis of an examination of the records of a single sawmill (*Sunds Sågverk*). The study is highly detailed and the archival work is careful and meticulous.

The most interesting original results consist of comparisons of the wage data in this study with those presented in the classic work, *Wages in Sweden, 1860–1930* (1933). Although the present work is somewhat critical of the choice of labor categories within the saw-goods sector made in *Wages in Sweden*, and therefore of the reported absolute level of sawmill wages, there is substantial agreement about the trend of wages (p. 162). Cornell also has a short but excellent section on the practical problems of distinguishing between skilled and unskilled categories of workers (pp. 169–72).

Despite these positive features and the generally high quality of the writing, however, the book is ultimately disappointing. To be blunt, much of the text is a boring litany of uninteresting detail. Devoting almost four hundred printed pages to this study can hardly constitute an efficient use of public funds. On a different level, the book raises serious questions about the author's training in economics. The most absurd example of economic illiteracy, unacceptable even in a principles course, is the assertion that "It was the strong demand for labourers which created the great supply of workers" (p. 385, English text).

A more serious problem arises relative to Cornell's choice of approach to measuring the living conditions of the workers. He rejects the "money" approach and instead opts for the "component" approach. That is, he attempts separately to measure a number of variables such as real income, unemployment, food consumption, housing, and health. He argues that such an approach is superior to the "money" one largely because it is impossible to put a money value on, or otherwise combine, a number of these variables. You cannot add "apples and pears" (p. 12). A major function of economics, however, is precisely to combine apparently disparate components by estimating their value to the consumer in money terms. Indeed the very same

problem considered impossible by Cornell has successfully been handled by Jeffrey Williamson in his recent work on Britain's "satanic mills." Using cross-section wage equations, Williamson is able to estimate the monetary value the workers themselves placed on the disamenities of industrialization and urbanization (see *Journal of Economic History*, March 1981).

Despite claiming that his various components cannot be combined into a single index of living conditions, the author himself makes an impressionistic attempt to do just that. His final conclusion is that while things may have been getting somewhat better during the 1860–90 period, it is "extremely doubtful" that the workers reached a "historically determined, minimum standard of existence." This apparently was so because their levels of food and housing consumption were not "acceptable" by contemporary standards (p. 337).

It is extremely difficult to know precisely what such an assertion means. Let it be noted, however, that the sawmill industry was among the most expansive sectors in a rapidly growing market economy. That simple fact, by itself, virtually guarantees that conditions were substantially worse in other major sectors of the economy.

LARS G. SANDBERG
Ohio State University

SVENBJÖRN KILANDER. *Censur och propaganda: Svensk informationspolitik under 1900-talets första decennier* [Censorship and Propaganda: Swedish Information Policy in the Early Decades of the Twentieth Century]. Summary in English. (Studia Historica Upsaliensia, number 121.) Uppsala: Acta Universitatis Upsaliensis; distributed by Almqvist and Wiksell, Stockholm. 1981. Pp. 219. 91.50 KR.

Like most dissertations, Svenbjörn Kilander's study of Swedish government information policy from 1905 to 1920 makes a modest contribution to our stock of useful information. At the same time, *Censur och propaganda* illustrates a familiar research problem, namely the selection of an effective interpretive framework.

Working from public and private archives, Kilander describes how the Swedish government, specifically the Foreign Ministry, became involved in manipulation of the flow of news across Sweden's borders. He shows that government activity began well before World War I and grew out of a need to stimulate Swedish exports and inspire confidence in those financial circles where Swedish industrialists sought loans. Peacetime news management typically took the form of positive propaganda. Wartime management included also negative censorship,

blocking out news in an effort to influence not only Swedish domestic opinion but also the leaders of European belligerent states who could cause difficulties if Sweden were perceived to depart from its officially neutral line.

The mechanisms of public intervention were formalized in 1909, when a secret press section was established in the Foreign Ministry to take over from private industrial initiative the promotion of commercial propaganda abroad. In return for monetary subsidies and priority access to telegraphic facilities, the private Swedish News Agency (*Svenska Telegrambyrå*) agreed to transmit government-supplied texts to its foreign subscribers. Additionally, the agency agreed to allow the Foreign Ministry to review all news dispatches received from abroad and to suppress any items deemed inappropriate for distribution to subscribing Swedish newspapers.

During World War I official news control functioned under the same agreement, enabling the Foreign Ministry to shape the news flow in the agency, rather than exercising direct censorship on the press. Controls were later tightened by an inter-Scandinavian agreement in 1916, a new system for censoring incoming and outgoing telegrams at the central telegraph office in Stockholm during 1917, and an agreement with the Social Democratic news agency in the same year. These arrangements were tested by the Swedish food riots in April and May 1917, which the government sought to muffle so as not to disturb trade negotiations with the Entente, and by the Luxburg affair in September 1917, involving relations with Germany as well. After 1918, news management reverted to positive commercial propaganda, now more openly acknowledged.

And finally to the matter of interpretation: Kilander has chosen to present these events as an illustration of Hilferding's theory that European states around 1900 were shifting from the laissez-faire policies appropriate for competitive commercial capitalism to direct service functions in support of monopoly finance capitalism. The adoption of news management tasks by the Swedish Foreign Ministry in 1909 must thus be related to the changing structure of Swedish industry. Kilander does not explore this connection in detail, beyond showing that government and industry were not on a collision course. Was the practice of news management something that originated outside the government, or was it more in the nature of a change of format in an older tradition so as to match the facts of telegraphy, rotary presses, and mass literacy? In considering the growth of Swedish public welfare policy, Hugh Heclo has posited a continuity of Swedish bureaucratic tradition which suggests that laissez faire was never as strong in that country as

public rhetoric might suggest. Whatever the case may be, Kilander has left us with some very open questions.

ROBERTA G. SELLECK
Harvard University Library

HÅKAN HOLMBERG. *Folkmakt, folkfront, folkdemokrati: De svenska kommunisterna och demokratifrågan, 1943–1977* [National Power, Popular Front, People's Democracy: Swedish Communism and Questions of Democracy, 1943–77]. Summary in English. (Studia Historica Upsaliensia, number 122.) Uppsala: Acta Universitatis Upsaliensis; distributed by Almqvist and Wiksell, Stockholm. 1982. Pp. 242. 99.50 KR.

Communist parties everywhere are constantly confronted with the problem of their dual nature—political organizations must exist and function in their national environments, and, at the same time, are members of an international movement that provides its claims and restrictions on the member "sections." The Swedish communists have labored under these conditions since World War II, and their troubles have been exacerbated by the existence in Sweden of a strong social democratic party (which controlled the government throughout much of this period) and a public opinion that was consistently pro-Western and, to a considerable measure, anticommunist.

For all communist parties, several major questions of policy have remained crucial. First of all, the communists must relate to the rest of the political system, which in Sweden can best be characterized as "bourgeois democracy." In this system, the communists face the problem of how to obtain political power, and, directly related to this, how the state and government apparatus should be organized after the takeover of power. In a pluralistic system, the communists must also devise ways to relate to parties and the electoral process. Finally, the question of civil rights and political freedoms for all segments of the population must be resolved. These problems confronted the Swedish communists as well in the post-World War II period.

The Swedish Communist party (SKP) approached these key questions in somewhat different ways in different time periods. During the period 1943–48, the SKP (in line with the policies sponsored by Moscow) advocated a "progressive" democracy, in which the communists would cooperate with the social democrats and other political groups to enhance democracy. This moderate approach came to a halt in late 1948; until 1956 the SKP adopted a much harder line against other political parties, including the social democrats. The emphasis on armed struggle as a necessary means to obtain

power was much more marked than had been the case in the earlier period. After the revelations of the Twentieth Congress of the Soviet Communist party (CPSU) in 1956, the SKP began to emphasize the "Swedish road" to socialism and communism; this road was perceived as primarily a peaceful one, and the party accepted the idea of some (albeit limited) cooperation with the social democrats. This period lasted until 1964. During the period 1964–69, a number of leftist groups were formed: while the SKP in 1967 changed its name to the "Left Party of Communists" (VPK), and fragmentation began to set in within the party.

One of the main aspects of the "Swedish road" had been outspoken criticism of the Soviet party and some elements of Soviet domestic and foreign policy. During the 1960s, this critical position toward the "socialist fatherland" created considerable strains in the party. At the same time, the emergence of China and its party as a major challenger to Moscow's position produced a pro-Chinese faction in the party itself. The result of these strains was a series of party splits, in which a pro-Chinese group established itself as a separate party (named SKP), while the pro-Soviet elements split out and formed the so-called Workers' Communist party (AKP). In addition to these developments, the new leftist groups underwent a series of splits and mergers, adding to the confusing diversification of the Swedish left. The VPK nevertheless remained by far the largest of these parties and groups.

Most Western observers of Swedish leftism have associated the VPK with "Eurocommunism," which combined a rejection of the CPSU and aspects of Soviet policy with relative tolerance of the "bourgeois" political system. Håkan Holmberg convincingly shows that this was not quite the case in Sweden. The VPK maintained its critical views of the CPSU and the USSR, but many leading elements in the party became increasingly radical in their condemnation of the "bourgeoisie" and the social democrats. The AKP, the so-called "traditionalists," advocated a moderate policy at home and staunch support for the USSR and the states of Eastern Europe in foreign policy. The SKP remained pro-Chinese and critical of the existing political system in Sweden. This constituted the essential configuration of Swedish leftism throughout the second half of the 1970s.

This volume is well researched and fairly well written. It adds to our knowledge about a communist party that has had some influence in the international movement, and in the process clarifies some aspects of Swedish politics of the left not well known outside of Scandinavia. The writing style is occasionally awkward, and it would have been preferable to have a more detailed discussion of the link

between Swedish leftism and the rest of the European left. Despite this criticism I find the book a valuable addition to the field and recommend it for those who can read Swedish.

There are detailed footnotes and an index of names. The bibliography is lengthy and valuable. There is also a short summary in English.

TROND GILBERG
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JUHANI PAASIVIRTA. *Finland and Europe: International Crises in the Period of Autonomy, 1808–1914*. Translated by ANTHONY F. UPTON and SIRKKA R. UPTON. Edited by D. G. KIRBY. (Nordic Series, number 7.) Minneapolis: University of Minnesota Press. 1981. Pp. xii, 270. \$29.50.

In recent years some important studies have reexamined the history of Finland in the period of autonomy, Osmo Jussila's and Robert Schweitzer's work come immediately to mind, and Juhani Paasivirta's book is a major contribution to our understanding of this period. It clarifies and sharpens the contours of various developments and changes in attitudes and opinions by focusing on a number of crisis situations in Europe and Finland. Paasivirta is concerned both with Finnish reactions to events in Europe and with European opinions about Finland. He differentiates carefully between the attitudes of the major social, occupational, ethnolinguistic, and political groupings in Finland. By connecting these with the structural changes taking place in the country and in international relations and political combinations, he explains a number of little-known phenomena.

Although Paasivirta deals with a multitude of issues, one is struck repeatedly by two recurring matters that should be obvious but all too often are not. For one, there are the similarities between Finnish considerations of Russia and Russian sensitivities in the nineteenth century and in the period since the Second World War. Even the specific tactics of handling the Russians are similar. Much of it is typical in any relationship between a weaker and a stronger partner in which the former is apprehensive of the latter. By simply changing the names of the actors in Paasivirta's text, one could thus sometimes apply it to other times and places. The second fundamental matter, which emerges time and again from the book, is connected with the above. This is that Finland concerned seriously besides Russia only Sweden. Still today these are the two most important countries for Finland. Both geographical proximity and historical associations contribute to this state of affairs.

Thus, Finnish reactions to crisis situations in

Europe reflected largely its considerations of Russia, and, outside the empire of the tsars, only in Sweden was there any abiding interest and sustained knowledgeable following of Finnish affairs.

Paasivirta's source base is extensive and varied running from official diplomatic communications to private letters, ballads, and newspapers aside from the relevant literature. The index includes only the proper names of persons, and the translation, which seems to have mainly aimed at being as faithful as possible, is occasionally too faithful to the original Finnish structures unsuitable for so close a rendering. The print is so small that, in his desire to save money, the publisher may have scared away potential readers. These are merely minor blemishes. As a whole this is both a first-rate synthesis and analysis by a mature scholar who has a discerning and sharp eye for the essential.

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HEINZ SCHILLING. *Konfessionskonflikt und Staatsbildung: Eine Fallstudie über das Verhältnis von religiösem und sozialem Wandel in der Frühneuzeit am Beispiel der Grafschaft Lippe*. (Quellen und Forschungen zur Reformationsgeschichte, number 48.) Gütersloh: Gütersloher Verlagshaus Gerd Mohn. 1981. Pp. 443. DM 150.

Heinz Schilling has written a superb case study of the role of religion in the conversion of the county of Lippe into a territorial state and of the resulting controversy that arose between the Count of Lippe and the city of Lemgo. The author examines the process of territorial state building, the function of Lutheranism in Lemgo and Calvinism in Lippe, and the outcome of Lemgo's resistance to the count.

Lippe began to develop into a territorial state during the last quarter of the sixteenth century. By that time Lemgo had already established its own Lutheran city church in the face of opposition from the count and from the estates. Thus, Lutheranism in Lemgo became symbolic of the city's communal independence within the county. When Count Simon VI began his rule in 1579, he set about to extend his authority by issuing a new law code, by reforming taxes, by recruiting bourgeois bureaucrats, and by establishing a new ecclesiastical structure. Simon's choice of Calvinism was political and pragmatic. Calvinism could serve to tie the small county of Lippe closer to the powerful Landgrave of Hesse. Also, as a Calvinist, Simon might be able to serve as a negotiator between the emperor and the Netherlands. Most important, however, the inevitable confrontation between the count and Lemgo

could be put off as long as there was the appearance of a central agreement on Lutheranism.

Simon VI introduced Calvinism by organizing a new consistory, by installing Reformed preachers, by ordering Calvinistic visitations, by establishing new schools, by staging theological disputations, by distributing Reformed catechisms, and by empowering ecclesiastical administrators. Simon's Second Reformation had none of the "typical" Calvinist emphasis on congregational governance. The new Consistorial Ordinance (1600) organized a bureaucratic ecclesiastical hierarchy leading from preachers and schoolmasters, through superintendents, to the consistory over which the count himself presided.

Lemgo saw Simon's Second Reformation as a violation of the city's independent status. Nevertheless, on June 9, 1609, the city accepted the so-called *Pfingstvertrag*, which established both Calvinism and the count's ecclesiastical jurisdiction within the city. On September 3, however, the citizens rose in armed revolt, elected insurgent leaders, and arrested those members of the city council who had supported the count. Schilling notes that the count's supporters were mostly first or second generation Lemgo residents. The rebel party, in contrast, was led by affluent Lutherans from ancient Lemgo families.

If Simon could have taken swift military action the insurgents would have had little chance. But as the leader of the Lower Saxon-Westphalian Imperial Circle, he was sworn to keep the peace and was therefore reluctant to resort to arms. Instead, the struggle took place in the imperial courts: in the *Reichskammergericht* at Speyer and in the *Reichshofrat* at Prague. Under both Emperor Rudolph II and Emperor Matthias, the courts decided in favor of Lemgo.

Legal proceedings were still underway in 1613 when Simon VI died. His successor, Simon VII, took a more conciliatory approach. The result was the *Röhrentrupper Rezess* (1617), which allowed Lemgo to retain Lutheranism.

Schilling's study shows that the political and social character of a given religious confession is determined as much by the social system and political situation within which it must operate as it is by dogma. Likewise, a prince's choice of a specific territorial religion is not necessarily related to doctrinal content. Elsewhere Calvinism might be associated with the struggle for freedom and democracy, but in Lippe Calvinism became an instrument for the extension of authoritarian rule. In fact, this study disproves the commonplace belief that Lutheranism sank into self-satisfied complacency after 1555. Indeed, Lutheran preachers were one of the driving forces in the rebellion against the count.

This work also illustrates the inherent opposition between urban and territorial interests within the polity of Lemgo. Confessional conflict created "vertical" social groupings. Members of the social elite were joined by small businessmen, artisans, and domestic servants in support of Lutheranism. They looked to Lemgo's tradition of independence from the count, and they resented the presence of a class of territorial civil servants within the city.

Finally, this book demonstrates the limits of the sovereignty of a count of a small territory. Neighboring princes and the emperor were trying to avoid the outbreak of open hostilities. In this situation the count had no choice but to reach a compromise with Lemgo.

Schilling's book is thoroughly researched, convincingly argued, and lucidly written. It is a major contribution to the historical literature of the Second Reformation on the eve of the 'Thirty Years' War.

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HENRY-RUSSELL HITCHCOCK. *German Renaissance Architecture*. Princeton: Princeton University Press. 1981. Pp. xxxiv, 379.

A book by Henry-Russell Hitchcock on the subject of German Renaissance architecture may come as a surprise to some readers because the author is mainly known for his very extensive work in the field of nineteenth- and twentieth-century architecture, especially American.

His goal, stated in the preface (p. xxix) is to provide a "comprehensive account" of German architecture from the beginning of the sixteenth century to about 1618, when the outbreak of the 'Thirty Years' War inhibited for many decades in Germany the transition to Baroque concepts, which were maturing in Italy at precisely that time.

The widespread and naturally heterogeneous material of German sixteenth- and seventeenth-century monuments is organized in ten chapters, of which the first three, introductory and general in outline, cover the period up to about 1550. Hitchcock contributes considerably to the understanding of the beginning and early development of Protestant church building by discussing the chapel in the wing of the Schloss Hartenfels at Torgau, erected by Konrad Krebs (1533-36). It was the first in a series of such structures frequently connected with castles, as for a long time former Catholic churches that could be converted were available (p. 104). The chapels of Schloss Neuburg a. d. Donau (pl. 121; pp. 203, 205; ca. 1538-43), Augustusburg near Chemnitz (Karl-Marx-Stadt; 1568-73; p. 161), and others

follow the pattern of the hall embraced by galleries or tribunes. But as the author stresses, they do not really establish a new type; only a feature like the pulpit placed above the altar in the center of the wall (Schmalkalden, pl. 267) is unmistakably Protestant (p. 215).

In subsequent chapters (4 through 6) Hitchcock attempts in his organization to reconcile the aspects of chronology and building categories by discussing first the castles that are subdivided into "Princely Schlösser" and "Lesser Schlösser" from about 1560 to 1580 (chaps. 4 and 5). He then devotes the first half of chapter 6 to the "Revival of Church Building in the Late Sixteenth Century," which is climaxed by the Jesuit Church of St. Michael in Munich. However, this section is somewhat "sandwiched," as the second half of chapter 6 is again devoted to castles, the "Major Schlösser of the Late Sixteenth Century."

Retardataire gothic churches, mainly but not exclusively favored by the Jesuits in the Rhineland (for example, at Molsheim, pl. 449; p. 352), as surviving examples of the outmoded, but only slowly abandoned style termed "Nachgotik" are referred to in chapter 8: "Northern Europe and Mannerism in the Early Seventeenth Century." The enlargement of the Residenz in Munich by the Elector Maximilian, who added the Italianate north front (pl. 365) and the Brunnenhof (pl. 364) to the existing structure, is of this period (p. 281), as well as the famous Friedrichsbau of the Heidelberg Schloss (pl. 366; 1601-07; p. 283) which includes a church-oriented parallel to the facade like Borromini's Oratorio dei Filippini in Rome. To the entry on the Friedrichsbau, a bibliographical reference to the printed dissertation by Wilhelm H. Köhler might be added ("Das Lusthaus Gottesau in Karlsruhe und der Friedrichsbau zu Heidelberg" [1965]). The exuberant form of decoration, of which the court facade of the Friedrichsbau is a "textbook example," found its reaction in a movement to which the author addresses himself in chapter 9: "The Academic Turn Against Mannerism." Its main protagonists were Joseph Heintz and Elias Holl, who with the Zeughaus in Augsburg (pl. 399; 1602-07; p. 312) and in particular with the Rathaus (pls. 415, 418) designed structures that, with their perpendicular drive and absolute clarity of articulation, mark the beginning of a new century.

The final chapter, "Religious Architecture of the Early Seventeenth Century," begins with a review of Catholic and Protestant church building and of the existing interrelationships. As one of the monuments destined to pave the way for the development of the German Baroque, the Hofkirche at Neuburg by Joseph Heintz and others (p. 340; pl. 436), begun in 1607 for Protestant use, played a leading role. Between the colossal, square piers the tribunes are

spanned above the aisles in such a fashion that they do not interfere with the skeletal impression of the interior, a system further refined in the Augustinian church at Polling in Bavaria (p. 347; pl. 453; 1621–37). With this structure especially, Hitchcock alerts his reader to a development beyond the strict limits of German sixteenth-century architecture, which came into full swing only after the 'Thirty Years' War—with buildings like the Jesuit church in Solothurn (1680–88) and the Cistercian church at Waldsassen (1685–90)—and continued in Germany and elsewhere far into the eighteenth century (see Heinrich Gerhard Franz, *Bauten und Baumeister der Barockzeit in Böhmen* [1963]).

Hitchcock's discussion of the famous pilgrimage church at Scherpenheuvel by Wencelas Coeberger (pp. 273, 346; 1609–27; no illustration) remains rather generic and does not take into account the important fact that the church—in Mariological symbolism a seven-pointed star—also determined the layout of the city, a seven-sided polygon with streets radiating from the church (see Georg Mörsch, *Der Zentralbaugedanke im belgischen Kirchenbau* [1965], pp. 21–82). But the author may have considered Scherpenheuvel, located in southern Brabant, as marginal to the objectives of his book.

In any event, Henry-Russell Hitchcock succeeds in keeping his reader's attention throughout the ten chapters and over three hundred sixty pages of his book. The organization of the unusual quantity of material proves to be exceptionally well considered. The presentation within the chapters maintains the form of a continuous text, which is well written and has been adequately illustrated. It provides the pertinent information concerning planning and building histories, patronage, and art historical settings. One feels sympathetically aware of the author's personal involvement in his chosen subject. If the field of German Renaissance architecture, far too long the domain of a limited number of specialists, finds wider recognition, a considerable degree of credit must be given to Hitchcock. The architectural monuments in Germany of the sixteenth century have found in him a distinguished advocate, and his book will fill a significant gap in a most satisfying and rewarding way.

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KARL HUFBAUER. *The Formation of the German Chemical Community, 1720–1795*. Berkeley and Los Angeles: University of California Press. 1982. Pp. viii, 312. Cloth \$40.00, paper \$14.95.

This monograph by Karl Hufbauer is a fine piece of historical craftsmanship. It pictures the formation of

a German chemical community in the eighteenth century as an intermediate stage between the definition of chemistry as a distinctive science, which had occurred in the seventeenth century, and the emergence of the chemical profession during the nineteenth century. Most previous analysts of the institutionalization of science have failed to recognize the "role of preexisting discipline-oriented communities" before the onset of professionalization (p. 2). In addition Hufbauer notes that these communities are often national in scope even in countries lacking a central government, as did Germany in the eighteenth century. Marshaling overwhelming evidence, he has no difficulty in convincing us that a vigorously nationalistic German chemical community came into being, but I would have liked him to pursue more deeply why it emerged. To be sure he does provide us with several good explanations in the form of moral, material, and manpower support for chemistry, which was rooted in Protestant culture and enlightened meliorism; and he points to the crucial role played by Lorenz Croll in founding Germany's first chemical journal, the *Chemische Annalen* in 1779. But I miss a discussion of the wellsprings and character of German nationalism in the eighteenth century and a more searching identification of the factors internal to the development of chemistry that caused German chemists to gravitate toward a national, as opposed to a regional or international, forum for scientific discourse.

While this German chemical community was forming, its cohesiveness was tested and ultimately strengthened as it rose to challenge Lavoisier's revolutionary theory of oxidation and his new concept of elements. Hufbauer ascribes the negative reaction of German chemists to the new French chemistry to nationalistic prejudice; more specifically, to loyalty to the phlogiston theory pioneered early in the century by a fellow German, Georg Stahl. Eventually, however, a few German chemists did embrace the oxidation theory and promptly challenged the chemical establishment. A sharp clash of views ensued, which ended in 1793 with a majority of German chemists crossing over to Lavoisier's paradigm. Hufbauer's subtle analysis of their conversion lends support to Thomas Kuhn's well-known characterization of scientific revolutions.

This study is a major contribution to the institutional history of science. Its scholarly apparatus is impressive. Three appendixes make up the last half of the volume, providing a most useful compilation of data, especially to historians of chemistry. The first presents sixty-five career profiles of chemists active in eighteenth-century Germany. The second reviews the development of chemical instruction in fifty-five institutions. And the third lists all subscribers to Croll's *Chemische Annalen*, their age, occupation, and place of residence. Although the book

lacks a standard bibliography, its footnoting is elaborate and comprehensive.

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PIERRE AYÇOBERRY. *Cologne entre Napoléon et Bismarck: La croissance d'une ville rhénane*. (Collection Historique.) Paris: Aubier-Montaigne. 1981. Pp. 415.

It is a curious fact that during the past fifty years French historians, who have collectively earned the reputation as the boldest of Clío's servants, have been so self-conscious and timid about their neighbor to the east. The reasons are not hard to find. Most of the military events around which modern German history has traditionally been cast—Jena, Waterloo, Sedan, the Marne, Verdun, the Blitzkrieg of 1940—were significant on both sides of the Rhine. Between 1871 and 1939, France judged itself, often harshly, by direct comparison with Germany. After 1945, it not only shared the burden of explaining the triumph of National Socialism but also remained haunted by a specter that Pierre Ayçoberry calls "le mystère de l'Allemagne" (p. 12). As he noted elsewhere (in *La question nazie* [1979]), the harder its intellectuals tried to demonstrate that Germans were complex the more they convinced the French public that they were abnormal—*les boches, les Nazis*. The publication of Ayçoberry's *Cologne entre Napoléon et Bismarck* therefore constitutes something of an exorcism. By adopting a research model well established for the social and economic history of French cities, he has written German history without reference to the German "problem."

The present volume is an abridged version of a nine-hundred-page thesis presented at the University of Lille in 1980. Unfortunately Ayçoberry's research has been mutilated in the reduction process. With the original notes stripped, the bibliography reduced to a single page, and the maps, tables, and graphs reproduced without mention of their sources, the book's value for specialists has been reduced. Although much of the standard thesis format remains, probably as an obstacle to the general reader, its heart—the careful reconstruction of demographic, occupational, and marriage patterns and economic structures associated with the students of E. Labrousse—has been removed. Whether the decision to publish the text in this manner was made by the publisher, the editors (Maurice Agulhon and Paul Lemerle), or the author himself, it is nonetheless regrettable. Pierre Ayçoberry is clearly an excellent scholar, a sensitive interpreter of German culture, and the obvious

successor to Jacques Droz as the preeminent French historian of Germany.

Ayçoberry is adept at blending the particular character of local history (and Cologne was a highly particularist place) with the general subject of the *Reichsgründung*. Cologne was a small, traditional Roman Catholic city when it was obliged to join Protestant Prussia in 1815. Over the next seventy-five years the consequences of state economic policy (freedom of navigation on the Rhine, the Zollverein, for examples) changed it into a modern industrial center. This transformation occurred in spite of resistance from all social classes. Ayçoberry's discussion of the character of local Biedermeier culture and the complex influence of religion on daily life is excellent. Historians of modern Europe should eagerly await his next publications.

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INA SUSANNE LORENZ. *Eugen Richter: Der entschiedene Liberalismus in wilhelminischer Zeit, 1871 bis 1906*. (Historische Studien, number 433.) Husum: Matthiesen. 1981. Pp. 264. DM 64.

Eugen Richter was always a difficult person to admire. Bismarck called him the "loudest rooster atop the progressive manure pile." Defenders of the government learned to fear his sharp-tongued and densely detailed criticisms, his exposures of budgetary waste, and his merciless attacks on militarism and colonial adventures. He alienated Catholics with his ideas about education, socialists with his opposition to welfare measures, conservatives with his unrelenting advocacy of free trade. Even those closest to Richter found him hard to live with. He was one of the most important reasons why the various left liberal factions remained at odds for most of the imperial era; a final integration of these groups took place only after his death. Richter has been no more congenial to historians than he was to his contemporaries. Conservatives and right-wing liberals denounced him as doctrinaire and unpatriotic. Left liberals, especially in the United States, objected to his antipathy to the labor movement and blamed him for splitting the German left. Marxists dismissed him as a personification of the *Kleinbürgertum's* opposition to social democracy. It was not surprising, therefore, that the Free Democrats, German liberalism's heirs in the Bonn Republic, named their scholarly foundation after Friedrich Naumann (whose liberal credentials are highly questionable) rather than Richter.

No doubt Richter was an exasperating character, a difficult friend, and an unbearable enemy. But he was also a dedicated parliamentarian, an exceptionally well-informed and energetic observer of Ger-

man society. Many of the things he stood for remain valuable today; much of what he opposed helped to undermine and eventually to destroy the Second Reich. Perhaps the time has come to take another look at Richter, to analyze again the sources of his support and the reasons for his failures, to reassess the strengths and limits of his political vision.

Ina Susanne Lorenz's *Eugen Richter* is neither a full-scale biography nor an exhaustive treatment of his public life. Instead, Lorenz examines a series of themes that illuminate what she calls "the determined liberalism" of the Wilhelmine age. She discusses Richter's views on such important issues as colonialism, social reform, social democracy, and taxation. She analyzes the content and style of his journalistic writings and political polemics. And she sketches his activities as a political leader, in the Reichstag and in his own party.

Lorenz has read widely in the secondary literature; her bibliography contains a helpful guide to Richter's own works. But she does not present the results of her efforts to full advantage. In the first place, her account is fragmented and difficult to follow. She divided her work into short, numbered sections (well over 100 in her 230 pages of text), a deplorable organizational device that continually breaks the line of argument and invites digressions and irrelevancies. More important, she does not articulate a unifying and analytical principle and fails to maintain a coherent narrative perspective. As a result, Richter himself often gets lost amid a profusion of separate subjects, secondary themes, and brief excursions. Lorenz leaves us with some interesting observations on the man and his times, but also with the conviction that a modern reevaluation of Richter's long political career remains to be written.

JAMES J. SHEEHAN
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ROBERT POIS. *Emil Nolde*. Washington: University Press of America. 1982. Pp. xxx, 263. Cloth \$22.50, paper \$11.50.

Several topics compete with each other in this psychohistorical study of Emil Nolde. First there is the Freudian-based analysis of Emil Nolde, the artist. Second is the attempt to use the insights gained from the analysis of Nolde to plumb "certain forces and trends in German cultural history." Third there is the effort to determine the degree to which Nolde's psychology is "representative of psychocultural problems arising in the course of German history." That last quote from the introduction means that Robert Pois has set himself the task of analyzing whether or not Emil Nolde's psychiatric makeup can help us understand the lure of National

Socialism for Germans, particularly from the lower middle class.

In pursuit of these themes, Pois has written a conventional Freudian analysis of Nolde, using Nolde's published works and unpublished letters made available to him by Nolde's family. The largest portion of the book is devoted to this task. Within this analysis of Nolde, considerable space is devoted to an explanation and argumentative defense of psychohistory as a legitimate segment of the historical discipline. Those unfamiliar with psychohistorical research would find these portions a worthwhile introduction.

The question that seems critical to this study is whether or not Pois has effectively used the insights unearthed from Nolde's psyche to explain the larger themes which frame his work, namely, the forces and trends of German history and, more specifically, the trauma of Nazism. Pois struggles with the critical hurdle, namely, the mass experiences in a society that contribute to mass psychology (because of Pois's reliance on Freudian psychology, this must be common experiences of generational cohorts).

Several such factors are suggested—the war, class experience, parental models. Unfortunately, these factors are not effectively plumbed. The war is mentioned only in passing. The class experience is implicit, as Pois uses the work of Adorno, Reich, and Horkheimer from the Frankfurt School. Weakest of all is the economic factor, with works of Pinson and Stolper used to give only the barest outline of the social and economic history of 1890–1930. Pois stumbles at the point where these experiences (or psychofactors) ought to produce the mass psychology similar, if not identical, to Nolde's individual psyche. He suggests this to be the case but undermines his point by not well correlating Nolde and the group he sees being there. The biographer of Nolde too often asserted Nolde's "uniqueness" and his artistic gifts as "mystery" to reassure the reader that Nolde reflects a common psychic path and that that path helps explain attraction to Nazism. The result is that the reader is left behind as the author makes the leap from one human being's psyche to the *origins* of group psychology.

Is, then, this psychohistorical effort unsuccessful? No, there is a segment of the book that this reviewer found rewarding and very helpful, namely, the insights into the expressionist movement that form another segment of the book. Here Pois is on far more secure grounds, and the reader's understanding of the expressionist movement is increased to the point of wishing Pois had made this the most important emphasis of his effort to use Emil Nolde as a guide into the mass psychology of Germans in the twentieth century.

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TOBIAS R. PHILBIN. *Admiral von Hipper: The Inconvenient Hero*. Amsterdam: B. R. Grüner. 1982. Pp. xiii, 231. \$24.00.

It is curious that there are no definitive biographies of any of Germany's naval leaders in the two world wars, figures like Alfred von Tirpitz, Reinhard Scheer, Erich Raeder, and Karl Dönitz. Tobias R. Philbin's work on Admiral Franz von Hipper begins to rectify this situation. Yet it must be emphasized that this is a professional, not a personal, biography, for there is only the barest outline of Hipper's life and personality. Nor is the subtitle—*The Inconvenient Hero*—really explained, though apparently it derives from a certain animosity toward Hipper on the part of Alfred von Tirpitz and from the fact that Hipper's rather equivocal role during the 1918 naval mutinies and his obscurity in the turbulent postwar years endeared him to neither the political right nor the political left.

It is in addressing specific topics that Philbin's work is most useful, though a lack of continuity sometimes results. His own service as a destroyer gunnery officer perhaps gives him greater insight into certain technical matters and into the navy as an institution than is true of other more exclusively academic historians, but some points are still arguable. As commander of the battle cruisers of the High Seas Fleet at the outbreak of war in 1914, Hipper grappled with the dilemmas of naval strategy. He advocated (improbably) using the battle cruisers for *Kreuzerkrieg* in the West Indies and the South Atlantic and recoaling along the coast of Iceland or Canada. Philbin details the concept even more sympathetically in a *Naval War College Review* article, pointing out that it was brought to fruition by Hipper's Chief of Staff, Erich Raeder, in 1939—but without noting that it was the adoption of oil that made refueling at sea truly practical.

Regarding the Dogger Bank engagement, the author takes issue with Arthur J. Marder and Otto Groos by defending the inclusion of the *Blücher* in the operation and arguing that the armored cruiser maintained squadron speed and that its guns had greater range than did the other battle cruisers. *Seydlitz's* near disaster also did not lead to the installation of antirash doors in the magazines, but rather to enforcing existing precautions. Philbin concurs with Antony Preston and Peter Padfield that at Jutland three British battle cruisers exploded, not because German AP shell was superior, as Siegfried Breyer has maintained, but because of unstable cordite and the lack of flash prevention policy.

As High Seas Fleet commander in 1918, Hipper planned a final sortie for October 30. Philbin indicates that Hipper understood the sortie had been approved, he was not party to any "admirals' rebellions," and, in planning to draw David Beatty's

Grand Fleet over minefields and U-boats, the operation was not merely a "suicide mission." But given his awareness of crew demoralization, Hipper's judgment in ordering the sortie was "a gross miscalculation" (p. 162), and mutinies broke out as a result. He was perhaps more sympathetic to the grievances of petty officers and crews than implied by Holger Herwig or Daniel Horn but had been unable to prevent the erosion of morale.

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JOHN WILLETT. *Art and Politics in the Weimar Period: The New Sobriety, 1917–1933*. New York: Pantheon Books. 1978. Pp. 272. Cloth \$17.95, paper \$8.95.

The world of cabaret and Caligari has proven so seductive that even historians of the renown of Peter Gay and Walter Laqueur have floundered in their attempts to reconstruct the intellectual and cultural life of the Weimar Republic. The drama of the period is so intense, its cultural life so varied and striking, that Weimar culture has served more frequently as a screen for personal or political projections than as a subject for serious historical analysis.

John Willett has not escaped the seduction of the myth of Weimar, and his sympathies for certain elements of the German left and his open defense of the values of avant-garde culture are present in almost every page of this work. But Willett's predispositions—unlike those of many of his predecessors—have allowed him to form a fresh and productive perspective on the period. He is quite uninterested in the academic scholars and expressionist film makers who have played such an important role in earlier accounts of the period. (Willett deserves considerable praise for writing what is undoubtedly the first book on Weimar culture in which there is no reference whatsoever to *The Cabinet of Dr. Caligari*.) But, these exclusions have allowed him to bring order to the confused interactions of those leftist artists and intellectuals who played such an important role in the cultural life of the period. Unwilling to dismiss as "radical chic" their attempts to combine politics and art, Willett has reconstructed the complex lines of intellectual influence and cultural organization that shaped the German left. In the process he has brought into much clearer relief the Berlin-Moscow cultural axis, which shaped the aesthetics of both Germany and the Soviet Union in the 1920s. Eisenstein and Piscator, Mayakovsky and Brecht, Meyerhold and Grosz, the Russian Constructivists and the German Bauhaus are all seen as products of a common central European culture, bound together by similar ideologies, constant exchanges of visits, and a complex network of cultural organizations.

Willett's emphasis on the Moscow connection has caused him to minimize unfairly the role played by the French in the creation of Weimar culture. The entire avant-garde spectrum in France from Picasso and the Surrealists to Cocteau and Les Six are treated as bourgeois dilettantes, whose aesthetic ambitions were crystallized in Art Deco. Yet, so much twentieth-century European cultural history has been composed from such a blatantly Francocentric perspective that even Willett's injustices to the Parisians are at times refreshing and illuminating.

The value of Willett's ideas and his impressive scholarship are not, however, well served by the form in which he has cast them. The work has neither the lucid style of a critical essay nor the scholarly citations of a monograph, and his sentences often require careful decoding before they yield their meaning. More information about the social infrastructure of the culture of the period would have strengthened his argument. And Willett's assumption that the creations of this period have left a lasting mark on Western culture seems a bit naive in light of recent devastating attacks on avant-garde aesthetics by postmodernist critics. In particular, his unquestioning defense of the projects of the Bauhaus seems more a product of the 1920s than of the last ten years.

But, *Art and Politics in the Weimar Period*, for all its Francophobia and aesthetic anachronism, greatly deepens our understanding of one of the most fascinating periods of all cultural history and serves to correct some of the errors of its predecessors, both popular and scholarly.

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JOHN P. FOX. *Germany and the Far Eastern Crisis, 1931–1938: A Study in Diplomacy and Ideology*. New York: Clarendon Press of Oxford University Press, for the London School of Economics and Political Science, 1982. Pp. ix, 445.

This fine study of German policy toward the developing crisis in East Asia from the Japanese occupation of Manchuria to the German decision to abandon its interests in China and side with Japan in the Sino-Japanese War is based on extensive research in German and British archives, published works, and materials of the International Military Tribunal for the Far East. In considerable detail John P. Fox reviews the internal debates over the appropriate policy for Germany to follow and stresses the concern of the German Foreign Ministry for maintaining a balance between China and Japan, the German War Ministry's great interest in building close ties to Nationalist China in order to secure raw

materials for German rearmament, and the efforts of Joachim von Ribbentrop—seconded by German ambassador to Japan Herbert von Dirksen—to carry through a policy of close alignment with Japan in accordance with the broader preferences of Adolf Hitler.

Readers will find a very shrewd discussion of the significance of Sino-German economic relations for both sides, though the author might have stressed the political interest of the Nationalists in showing other powers that if German citizens and trade could prosper in the China market without the extraterritorial rights Germany had been obliged to surrender, surely they could too. On the basis of new Japanese research and the papers of the German intermediary Friedrich Wilhelm Hack, Fox provides an account of the origins of the Anti-Comintern Pact, in which he suggests a far more active role was played by the Japanese than hitherto believed. We are also given new insights into the repercussions of Germany's racial policies on its relations with Asian nations. The book is especially good in delineating the differences between the German Foreign and War Ministries over the extensive projects of the latter in China, though we never do get a clear picture of what those projects finally amounted to. An interesting puzzle is raised by Japanese soundings for a written military agreement with Germany early in 1937: was this connected with Japan's China policy at the time?

The author is convincing in his description of the problems posed for Germany by the outbreak of open hostilities in July 1937 and in his account of the infighting over this issue, with von Ribbentrop—who in the imagination of some historians was working on an alliance with England—deeply involved in the details of German-Japanese relations. There is an excellent survey of the abortive German attempt at mediation followed by Germany's dramatic turn to Japan. Fox's account of the speeding up of this process in early May 1938 in order to have Japan join Germany in pressure against England fits in with the contention that it was at that time that Hitler decided to have his first war later that year.

The endless arguments over the projects of Ferdinand Heye in Manchuria probably do not deserve the space the author allots them, while Hitler's preference for a state trading monopoly with China (as with Spain) deserved more attention. Students of National Socialist Germany and of the diplomacy of East Asia will turn to this book for enlightenment for years to come. And they will see in detail how in this field as in so many others the ideological goals of National Socialism came to take precedence over both the goals of traditional diplomacy and economic interests.

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MAX P. BIRNBAUM. *Staat und Synagoge, 1918–1938: Eine Geschichte des Preussischen Landesverbandes jüdischer Gemeinden, 1918–1938.* (Schriftenreihe Wissenschaftlicher Abhandlungen des Leo Baeck Instituts, number 38.) Tübingen: J. C. B. Mohr (Paul Siebeck). 1981. Pp. xii, 298. DM 98.

Meticulously documented, drawing on the unique personal experiences and contacts of the author, sensitively presented, *Staat und Synagoge* nonetheless raises questions concerning its *raison d'être*. Does the overwhelming tragedy visited upon the Jews of Germany legitimize the notion that anything they did collectively or individually deserves a monograph? This history of the Prussian State Association of Jewish Communities from 1918 to 1938 would suggest not. Max P. Birnbaum was the last deputy general secretary of this purely administrative body. He describes the petty personal rivalries and mentions—without analyzing—the more important political-religious conflicts that prevented German Jews from achieving a national unified organization (until virtually forced to do so by the Nazis). In this vacuum and because 75 percent of Germany's Jews lived in Prussia, the Prussian association assumed some representative functions. There is little evidence, however, that its actions ever compelled much interest even from Prussia's Jewish population. In the twenties the association's main effort went into dickerings with state bureaucrats for monetary support proportional to that doled out to the churches. Originally a matter of principle, this bargaining became a necessity because of the devastating effects of the inflation on the preponderantly middle-class Jewish community and the erosion of the funding base for Jewish institutions. Leaders of the association met with ill will, foot-dragging, and hostility from German bureaucrats, a response Birnbaum accepts rather than explains. Recognition of the synagogue's equality with the churches remained incomplete, a matter for negotiation and maneuver instead of a clear right. The distribution of state funds, details of internal governance, certification of teachers, support for small, failing rural congregations, aid to retired rabbis or their widows, occupational restructuring for Jewish youth, and the constantly renewed attempts to achieve national organizational unity provide Birnbaum with his themes. These same activities continued with a new urgency but in an increasingly hopeless situation under the Nazis.

Birnbaum's monograph is out of place in the Leo Baeck Institute's excellent series. Although careful and fair-minded, it is simply too narrow to be useful to any who lack a fund of specialized knowledge. The heated controversy over Jewish identity in the Weimar period, the bitter religious debates between orthodox and liberal Judaism, the ideologi-

cal arguments of Zionist, Assimilationist, and German Nationalist Jews, the problems posed by a declining birthrate and an increase in mixed marriages, the desperate attempts to defend against antisemitism, the nature of German antisemitic politics, the strategies to be adopted after January 30, 1933—all these vital and substantial issues Birnbaum reduces to their "bureaucratic significance" or skirts altogether. His book tells the story of German Jewry in its era of crisis with all the interesting parts left out.

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HELMUT KRAUSNICK and HANS-HEINRICH WILHELM. *Die Truppe des Weltanschauungskrieges: Die Einsatzgruppen der Sicherheitspolizei und des SD, 1938–1942.* Part 1, *Die Einsatzgruppen vom Anschluss Österreichs bis zum Feldzug gegen die Sowjetunion: Entwicklung und Verhältnis zur Wehrmacht*; part 2, *Die Einsatzgruppe A der Sicherheitspolizei und des SD, 1941/42: Eine exemplarische Studie.* (Quellen und Darstellungen zur Zeitgeschichte, number 22.) Stuttgart: Deutsche. 1981. Pp. 687.

Most historians are probably inclined to doubt the desperate need for another 700-page tome on what was in retrospect a relatively minor part of the Nazis' killing and terror machine, the so-called *Einsatzgruppen*. Though in formal existence for some six years, these mobile killing units were active for a little more than two years before they were replaced by the more efficient, stationary operations at Auschwitz and Treblinka. What contemporary documentation exists about *Einsatzgruppen* has been available for some time, and both the groups' activities and their role in the larger evolution of the Holocaust have been discussed in some detail in earlier scholarly and popular publications. The question as to whether another study can do more than synthesize the earlier accounts is, then, quite legitimate.

Fortunately, the book under review quickly dispels the initial doubts. The volume is unusual in a number of respects. To begin with, it really consists of two separate books of rather unequal length. The author of the smaller part, Helmut Krausnick, the former director of the Institute for Contemporary History in Munich, presents an overall introduction to the *Einsatzgruppen*. He describes their specific position and function in the Nazi terror machinery, as well as their relationship to other powerful interest groups in the Nazi system. With regard to the latter, Krausnick concentrates on the relations between these SS (*Schutzstaffel*) units and the Wehrmacht in occupied Poland and Russia. Hans-

Heinrich Wilhelm is a much younger scholar, and his contribution to the book, a detailed study of *Einsatzgruppe A*, is a revision of his recent dissertation. As is to be expected when one volume contains the contributions of a retired scholar and a recent PhD, the historiographic emphases of the two authors differ considerably. Krausnick's part reflects the interests in administrative and organizational history of the "older" school of historiography, while Wilhelm emphasizes socioeconomic and ideological aspects of the subject. Wilhelm's part also contains reprints of a number of previously unpublished documents, for the most part their provenance being the *Einsatzgruppe A* itself.

Despite its position in the volume, Helmuth Krausnick's essay is less an introduction to the subject than an impassioned tour de force on one specific aspect of the problem, the *Einsatzgruppen*'s relationship to the army during their joint operations in the eastern theater of operations. In doing so Krausnick is vigorously defending a specific thesis or, more precisely, demolishing a previously held hypothesis. Both during the war crimes trials and subsequently in numerous memoirs, former Wehrmacht leaders maintained that they either knew little or nothing of the *Einsatzgruppen*'s activities in the east, or if they did know about them, regular army units certainly did not aid in the killing operations. Krausnick's object is to expose the "clean moral shield" of the old Wehrmacht as a sham. Wilhelm's concerns are broader and more related to "newer" questions posed by historians of Nazi Germany. Using the history of *Einsatzgruppe A* as a case study, Wilhelm analyzes not only the activities of the unit but also the self-image and motivation of its leaders. The latter aspects are of particular interest in the case of the *Einsatzgruppen* since, unlike most Nazi organizations, their leaders were well-educated and came from the upper socioeconomic levels of German society. (Of the fourteen commanders of these units, no fewer than six had PhDs.) The *Einsatzgruppen*, then, present Nazism in an unusual form that bears little relationship to the traditional image of lower-middle-class misfits motivated by socioeconomic resentments and failures.

In terms of methodology the book presents a number of important innovations. Both authors, but particularly Wilhelm, make effective use of postwar Allied and West German trial documentation. (From the evidence in this book, it is clear that these sources contain a great deal of important information. It is unfortunate that historians to date have made relatively little use of them.) Wilhelm also uses biographical information to good effect to form collective psychosocial portraits of the unit's leadership corps. Finally, the book is a textbook model in the qualitative evaluation of suspect statistical sources.

The leitmotiv of both authors' contributions is the question of how the *Einsatzgruppen* were able to carry out their grisly tasks. Krausnick raises the question negatively, that is, why did the army not prevent the murders when it was clearly in a position to do so? Wilhelm poses the same query positively; that is to say, how were the *Einsatzgruppen* themselves motivated to attempt to kill hundreds of thousands and how were they physically able to do so? To take the last question first, Wilhelm clearly demonstrates the crucial role of "native" helpers (the so-called *Hilfswilligen*, *Hiwis*) among the occupied peoples (especially Estonians, Lithuanians, and Ukrainians) in carrying out the mobile killing operations. The Nazis used a relatively small number of German SS troops; most of the actual killings were done by collaborationist auxiliaries. This in no way relieves the Nazis of the responsibility, but it is clearly an aspect of the problem that has received too little attention among historians. As for the motivation of the German perpetrators themselves, both Krausnick and Wilhelm, somewhat surprisingly perhaps, emphasize the ideological component of the subject. Krausnick has little difficulty in revealing numerous spots on the Wehrmacht's "clean moral shield." To be sure, regular army units did not, for the most part, actively participate in the killing operations; however, aside from a few notable exceptions, the field commanders and the officer corps in general not only knew of the *Einsatzgruppen*'s activities, but attempted to justify these crimes to the troops under their command. Moreover, the army leaders were motivated far less by fear or even opportunism than by ideological agreement with the Nazis' overall aims. Krausnick demonstrates that the traditional conservatives, who still dominated the officer corps, to a large extent accepted Hitler's view that the war in the east had to be fought according to different rules than in the west, since in Poland and Russia Germany was fighting uncivilized barbarians. As far as Poland was concerned, the traditional anti-Polish prejudices of the still largely Prussian officer corps fit in well with the Nazis' racial theories, while in Russia most army commanders all too readily accepted the Nazis' thesis that the "immoral" Bolshevik system was essentially created and controlled by Jews. As a result the wholesale murder of Jews could be "understood" as eliminating the ruling class of an immoral enemy.

The army commanders "understood" the necessity of mass killings, but the leaders of the *Einsatzgruppen* were in active command of the killers. What motivated them? Wilhelm, who devotes a great deal of attention to this question, shows that here, too, ideological considerations played a decisive role. The officers of the *Einsatzgruppen* were not social failures in the usual sense, though a number

might well be considered psychosocially maladjusted. An almost universal characteristic, however, was their early and fanatic commitment to Nazism. Virtually all of the commanders had joined the Nazi party before 1933, most had served in various Freikorps and Heydrich's *Sicherheitsdienst*. Many had resigned their memberships in the Christian churches in order to demonstrate their commitment to Himmler's obscure Germanic religion. In short, the *Einsatzgruppen* leaders saw themselves as personifications of the new Germanic order, representatives of a master race sent out to administer a vast hinterland peopled by dangerous but inferior races.

This self-image readily enabled the *Einsatzgruppen* leaders to relativize their killing activities as a small, though necessary, part of their work in Poland and Russia. They were responsible for "securing" the Reich's new Lebensraum, and that meant murdering several hundred thousand "enemies" (90 percent of whom were Jews), but in their reports and concepts the *Einsatzgruppen* were more concerned with the "positive" aspects of pioneering the rule of the master race: "guiding" various collaborationist political groupings, bringing German culture to the barbarians, and so forth. And in a perverse way the *Einsatzgruppen*'s self-image was even correct. Their killing activities did take far less time and effort than the other aspects of their operations.

It is clear, however, from Krausnick's and (especially) Wilhelm's analyses and documentations that the members of the *Einsatzgruppen* were far from living up even to their perverse standards of new morality. In fact, the *Einsatzgruppen* were not only ineffective (except for their killing operations), but were cesspools of quite ordinary corruption. Lack of discipline, financial irregularities from blackmail to embezzlement, and numerous acts of "normal" criminality (rape, theft, nonpolitical murder, and so forth) were widespread among the members of Heinrich Himmler's new order. Numerous *Einsatzgruppen* members even violated the most basic of Nazi standards, the laws against sexual contact between "Aryans" and other races, apparently convincing themselves that the Nuremberg laws applied only in the Reich and not in the occupied areas. On balance, the *Einsatzgruppen* lived up to no ideal, not even their own. On the contrary, they were inefficient, greedy, cruel, self-centered men who used their power largely for purposes of oppression and self-aggrandizement. Their activities were also singularly ineffective. The wholesale slaughter and insensitive policies quickly alienated in Russia virtually all potential collaborators (the Nazis had no wish for collaboration in Poland), gave added impetus to the partisan movement, and consequently hastened the defeat of the Nazis.

Both authors present their findings dispassionately, completely, and with exemplary attention to the

mechanics of the historian's craft. Clearly, this is the definitive volume on an important aspect of the Nazi phenomenon. Although it seems almost petty to mention them, a few minor caveats and criticisms should be noted. Krausnick's contribution is especially influenced stylistically, perhaps overly so, by the Institut für Zeitgeschichte's *amicus curiae* briefs. The work is massively documented, but also a bit cumbersome and saddled with an overabundance of snippet quotations. The symmetry between the two parts of the book is not perfect. Wilhelm approaches the topic more broadly, but concentrates on one *Einsatzgruppe*. While Krausnick covers all of the groups, he clearly emphasizes one substantive aspect of the subject. In addition, the origin of Wilhelm's contribution as a PhD dissertation is a little too evident. In an effort to provide as much background as possible on the internal conditions in the occupied areas, Wilhelm provides a bewildering array of information, much of which is already available in previously published English- and German-language works. A little more synthesizing would have made the patterns clearer. Finally, there is the problem of the extensive documentation interspersed with Wilhelm's narrative. It is true that the excerpted documents do provide insight into the *Einsatzgruppen*'s mind-set, but the material undoubtedly raised the book's price to exorbitant levels as well.

Still, these are minor criticisms. The book provides not only a great deal of substantively new information, but also demonstrates how new insights are facilitated when familiar material is subjected to new questions. In this sense perhaps the book's most important contribution is implicit rather than explicit. It is related to the familiar historiographic controversy over the degree of ideological commitment as a motivating factor for the Nazis' behavior. Krausnick and Wilhelm demonstrate that, subjectively at least, the fervent Nazis that led the *Einsatzgruppen* were convinced ideologues who actually believed in their perverse mission.

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PAULA SUTTER FICHTNER. *Ferdinand I of Austria: The Politics of Dynasticism in the Age of the Reformation*. (East European Monographs, number 100.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1982. Pp. 362. \$27.50.

The tendency of professional historians to slight the career of Ferdinand I has long been lamented by Habsburg specialists. Victor S. Mamatey has described the frequently neglected younger brother of Charles V as "the true founder of the Austrian

Habsburg empire," while Robert A. Kann has unequivocally asserted that "in the evolution of the Habsburg empire Ferdinand I is perhaps the most important figure. A major modern biography is needed." Paula Sutter Fichtner has responded to this need with a meticulously researched volume focusing on Ferdinand's overriding preoccupation: the advancement of his family as a sovereign dynasty. His acquisition of Bohemia and Hungary, his endless conflicts with the Turks, and his vigorous responses to the Protestant challenge all took place, as Fichtner has carefully demonstrated, within the context of his consuming ambition to expand the Habsburg *Hausmacht*. "The relentless consistency" with which Ferdinand pursued family interests extended to his own children, for whom he functioned as a determined if somewhat protective matchmaker. Yet dynastic commitment could not prevent—and in fact exacerbated—serious tensions with his son Maximilian over the young man's pro-Protestant feelings; neither did it significantly relieve his strained relations with Charles V, whose lack of support for Ferdinand's interests in east-central Europe led to a breakdown in communication between the two brothers during the early 1550s.

Fichtner has treated the various nuances of these manifold problems in a painstakingly detailed fashion. Her chapters on Hungary and Bohemia seem more interesting than those dealing with German affairs, perhaps because they cover less familiar issues and themes. Fichtner's style, though clear and precise, has a mechanical quality, so that she often makes a systematic presentation of momentous events without bringing them to life. She describes many facets of Ferdinand's character and private existence, but never manages to make him emerge from the pages of her book as an animated, flesh-and-blood human being. In short, Fichtner has produced a technically competent monograph, replete with extensive notes, bibliography, and index, and visibly marred only by an unnecessarily large number of typographical errors. Despite her very solid contribution, the modern biography for which Robert A. Kann has pleaded remains to be written.

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THOMAS M. BARKER. *Army, Aristocracy, Monarchy: Essays on War, Society, and Government in Austria, 1618–1780*. (East European Monographs, number 105; Brooklyn College Studies on Society in Change, number 16; War and Society in East Central Europe, number 7.) Boulder, Colo.: Social Science Monographs; distributed by Columbia University Press, New York. 1982. Pp. ix, 290. \$20.00.

This collection of Thomas M. Barker's recent essays and monographs is welcome for many reasons, not

least because it is more than a mere sum of its parts. Although the author modestly claims this to be no more than a progress report on a more ambitious undertaking, a comprehensive social history of the Austrian monarchy from 1618 to 1740, the book stands on its own as a study of the Austrian military elite in that period, surely a major component in any larger social analysis. All of these pieces have appeared elsewhere, some only in German, but most of them have been substantially reworked to make them fit together in this volume.

The essays included here alternate between considerations of larger cognitive and structural issues, based on an impressive grasp of the theoretical work in a variety of disciplines, and monographic case studies of shrewdly selected individuals and groups within the "strategic elite," that group of some two hundred families who for six generations provided the social, political, and military leadership cadres for the Austrian monarchy, monopolizing in turn the rewards handed down for such service. Examining the careers of Montecuccoli, Piccolomini, Lobkovic, and Daun, Barker traces the transition from the private military entrepreneur to the eighteenth-century beginnings of a professional, state-oriented service. The Piccolomini essay provides occasion for a brave dip into historiographically troubled waters—the Wallenstein murder—from which Barker emerges with some illuminating conclusions and refreshing good sense. In the case of Lobkovic he convincingly documents the relatively minor role played by military professionals in the political apparatus. In all of the essays there is a wealth of detail from manuscript sources, particularly the valuable family archives now concentrated in regional Czechoslovakian depositories.

The concluding essay on military history and the social sciences presents few conclusions related to the Habsburg monarchy in particular but does offer a useful agenda for those who wish, as we all should, to move beyond the traditional study of strategy and campaigns to look at the problem of war in its larger social context.

Since these essays were written over a decade or more for a variety of purposes and audiences, there are bound to be minor inconsistencies. In most cases they are addressed to scholars who already know the period well. In spite of the book's title, Barker says little about the monarchy itself or the civilian ministers who served it. The role of the emperors is never made entirely clear, though war and justice were universally recognized as the fundamental functions of the crown in all European societies. The peculiar style of Habsburg absolutism surely had something to do with the cosmopolitan character of its military elite.

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GUNTHER E. ROTHENBERG. *Napoleon's Great Adversaries: The Archduke Charles and the Austrian Army, 1792–1814*. Bloomington: Indiana University Press. 1982. Pp. 219. \$18.95.

"In Deinem Lager ist Österreich," wrote the poet Grillparzer referring to octogenarian General Wenzel von Radetzky and his troops during the 1848–1849 Revolution. The same thing might be said, although in a less obvious historical context, about the role of Austria's soldiers during the stormy years between 1792 and 1815. Perhaps the single most important point made by Gunther E. Rothenberg is that the constant concern of the archduke and his successors after the debacle of 1809—the military diplomat, Prince Karl von Schwarzenberg, and his strategic mentor, Radetzky—was the preservation of the dynasty's martial might as the main link uniting the multinational state they served. Moreover, the *kaiserlich-königliche Armee* could not be turned into a superbly efficient instrument of compulsion like its French and, after 1806, Prussian counterparts. This was because Charles, his brother Emperor Francis (II) I, and other Habsburg leaders, notwithstanding their mutual suspicions and rivalries, were of one mind in rejecting the thought of basic social and political reforms that might have enabled them to exploit an aroused popular will. There were no von Steins, Scharnhorsts, or even a Frederick William III. The changes that were effected in order to meet the French challenge constituted mere tinkering, however necessary, of a technical, military nature. They did not provide a basis for implementing the novel, sweeping Napoleonic style of warfare, for which, in any case, Austria seemed to lack the necessary reservoir of highly talented commanders. The cautious, deliberate, methodical mentality of the eighteenth century continued to hold sway.

Charles, whose equestrian statue still shares the place of honor with Eugene of Savoy in front of Vienna's *Hofburg*, became the subject of patriotic myth-making in the later nineteenth century. While he is careful to single out the archduke's undoubted strengths, such as personal bravery and an intellect superior to that of most of his compeers, Rothenberg straightforwardly debunks the officially inspired hagiography. Although regularly confronted by the mistrust and interference of his imperial brother and by hardly less frequent aulic cabals, Charles displayed many a personal weakness in the course of his relatively short career: occasional nervous disorder, poor judgment in the choice of intimate associates, inadequate rapport with other high-ranking generals, and grievous operational errors. Of course, to quote an old Austrian proverb, among the blind the one-eyed is king. Rothenberg's scathing analysis of the generalship of the archduke's prime competitor, *Feldmarschall-Leutnant* Karl

Mack, in the 1805 campaign supplies all the evidence needed.

If the Habsburg host ultimately emerged from the French and Napoleonic Wars with credit, it was more the doing of the enlisted men and the lower-level officers who performed well notwithstanding frequent want and extreme stress. One can only surmise that their willingness to stand and die by the thousands was due to the psychological bonding processes that characterize military institutions—phenomena much better understood today after more than three decades of social scientific research.

This tightly and engagingly written book likewise contains much material that will delight the student attracted to the traditional variety of military history. The study provides detailed data on the tactics, weaponry, uniforms, training, daily life, logistics, social origins, recruitment, finances, administration, and organizational components of the Austrian army. Equipped with many illustrations, charts, and good-quality maps, it also incorporates clear accounts of campaigns and major battles, especially Charles's flawed victory over Napoleon at Aspern-Essling and his final, if for the Corsican costly, defeat at Wagram. The notes and the bibliography make it abundantly clear that Rothenberg, a well-seasoned ex-soldier himself, has researched his topic punctiliously. He has not only sifted a plethora of printed references but has also plumbed archival records in London (diplomatic reports in the Public Record Office) and Vienna, thus offering fresh insights. The volume represents a genuinely significant addition to our knowledge of the period and the subject, especially from the vantage of civil-military relations. It fully supersedes the superficial work of Manfred Rauchensteiner, *Kaiser Franz und Erzherzog Karl* (1972).

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JAMES SHEDEL. *Art and Society: The New Art Movement in Vienna, 1897–1914*. Palo Alto, Calif.: SPOSS. 1981. Pp. ix, 232. \$27.50.

In his brief account of the Austrian Secession, James Shedel rightly emphasizes the Secession's place within a broadly European context of art nouveau, while sketching the emergence of modern art in Vienna between 1897 and 1914. Shedel's chapters deal in turn with the secession of Gustav Klimt's *Jungen* from the *Künstlergenossenschaft* in 1897, with the attempts of the Secession to influence and improve the society around it, with Klimt's University paintings and the new *Klimtgruppe* of 1905, and with the next generation of the avant-garde (Egon Schiele, Oskar Kokoschka, and Adolf Loos). Shedel is concerned here not with art history but rather with "the impact of the Secession upon the popular, official, and cultural environment of which it was a part" (p.

4), and his book recalls the huge role that the search for institutional success and influence played in Austrian intellectual life.

Shedel's contribution lies in his discussion of the politics and practicalities of the Austrian art world. It is surprising to see the degree to which the Secession appears here as a social reform movement (with an almost positivist view of the rational and constructive role of art in society), even though the story that unfolds is one of rapid cooption and failed ideals. There was apparently not much self-irony in the idealism of these painters and architects, either in their critique of the careerism and materialism of historicist artists or in their need to rebuild Vienna to their tastes. Yet the Secession encountered success with the wealthy and the influential almost from the outset, and its exhibitions were greeted with popular enthusiasm and critical approval. In fact, the Secession became an establishment so quickly that within a few years Karl Kraus could regard it as an exemplar of artistic corruption. Shedel tells this story almost without comment, and the reader is left to wonder if this is the normal fate of modern art movements or a special feature of Vienna's highly institutionalized cultural life. Here, comparisons with Paris or with the secessions in Munich and Berlin might have been helpful.

Shedel's book is a serious, responsible effort, and it is enhanced by a large number of valuable photographs. But *Art and Society* suffers from the inevitable comparison with Carl Schorske's more elegant and complex discussion of these issues in *Fin-de-Siècle Vienna*. Shedel's book sometimes moves slowly since he assumes a reader to whom Hermann Bahr and *Jung Wien* are unknown, and his prose suffers from repetition and careless editing. Moreover, his argument would have been improved by a fuller discussion of the historical literature and the nature of his own contribution. Although he acknowledges his debts to Schorske as well as to Peter Pareit and William McGrath, he might have shown how his account fills in the everyday realities of Viennese cultural politics that lie between Schorske's portrayal of Klimt and the emphasis by Allan Janik and Stephen Toulmin on the importance of Kraus for the succeeding generation of the avant-garde. In fact, Shedel's argument grows more interesting as he moves beyond the Secession to Schiele and Kokoschka, but he does not think through these questions of art and ideology in a sufficiently coherent and explicit way. Thus, it is never quite clear whether Shedel agrees with Loos and Kraus that the Secession was a "cultural sham" (p. 170), and Shedel leaves unstated the book's implied thesis that the passage from 1897 to 1914 should be regarded as an artistic ascent and a moral decline.

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BRUNELLO VIGEZZI. *Il PSI, le riforme e la rivoluzione: Filippo Turati e Anna Kuliscioff dai fatti del 1898 alla prima guerra mondiale*. (Biblioteca dell' Istituto Socialista di Studi Storici.) Florence: Sansoni. 1981. Pp. x, 208. L. 18,000.

For the past thirty-five years the craft of history in Italy has been dominated by leftist historians who have done yeoman work in contributing to our understanding of modern Italian society. There is one area, however, in which this leftist (not necessarily Communist) historiography has fallen short: the history of the Italian Socialist party (PSI) from its foundation to the Fascist period. Interpretations based upon misunderstandings and a desire to serve current political aims abound. Writers repeat adverse judgments without a thorough knowledge of the facts or of the historical contexts. The leaders of the reformist wing, Anna Kuliscioff and especially Filippo Turati, are the persons whose reputations have suffered the most.

During the past few years attempts have been made to examine reformist policies in a more objective and dispassionate manner, a development spurred by the appearance in 1977 of the final six volumes of the Turati-Kuliscioff correspondence after a hiatus of almost twenty years. In addition to books and articles, scholars have participated in conventions and discussions on reformist leaders as the fiftieth anniversaries of their deaths have approached.

Part of the renewed interest in understanding the reformists, Brunello Vigezzi's book is based primarily on the Turati-Kuliscioff *Carteggio* but also demonstrates a remarkable command of the secondary literature. The book is divided into two sections: a discussion of reformist action between 1898 and 1915 and an "appendix" consisting of Vigezzi's comments at several conventions over the past several years.

"It is incredible how a few ideological taboos have for so long blocked important research, forcing us to rehash trite controversies" (p. vii). Rather than going into the details of major issues himself, Vigezzi focuses upon the most serious misconceptions regarding the reformists and indicates the neglected areas to which scholars should direct their efforts.

First the myths: Turati was so pro-Giolitti that he transformed the PSI into a satellite of the democratic liberals over the protests of Anna Kuliscioff. But Vigezzi demonstrates how Kuliscioff was friendlier to Giolitti and cites evidence showing how strongly Turati opposed Giolitti on important occasions. Based on realistic assessments, reformist tactics constantly adapted to new situations—an obvious fact of political life that some historians choose to ignore, tied as they are to Antonio Gramsci's old saw about a Turati-Giolitti "partnership." Another myth con-

cerns the supposed reformist neglect of the peasants and the exclusive concentration on urban workers, but Vigezzi presents Anna Kuliscioff as emphasizing the paramount importance of the peasants (pp. 38–39). Still another myth depicts the reformists as having been caught unawares by the economic crisis of 1912–13, which supposedly did them in; but Vigezzi cites Turati's cogent analysis which proves the opposite (pp. 66–69). Finally, the reformists are condemned for the PSI failure to prevent Italy's entrance into World War I, but the PSI did not follow the reformist line then. Using strong language, Vigezzi labels this particular myth "a pure and simple historical fraud" (p. 184). Other examples of this kind of lore could be cited.

Vigezzi appeals to historians to consider reformist policies within the context of their time, believing that the politics of Turati and Kuliscioff made great sense, even if they made mistakes (usually different from those generally attributed to them). More objective and wide-ranging analyses of reformism are needed in order to assess correctly the Socialist impact on Italian history. For example, Mussolini's shift from neutralism to interventionism has yet to be studied properly, that is, with an emphasis upon the problems of the PSI after the beginning of World War I, problems which remained after Mussolini left the party. The still-debated problem of achieving socialism while preserving individual freedom—at the center of the Turati-Kuliscioff dialogue—has not been investigated despite the considerable light such research could shed on the issue. Reformist attempts to strengthen the Italian parliamentary system have been neglected, despite the role of antiparlamentarianism in the suppression of Italian democracy. The real reasons for Turati's policies during the postwar era are still unknown, and here we are dealing with the immediate situation that brought fascism to power.

Vigezzi's book should be read by all scholars who wish to gain some perspective on the tormented but crucial history of Italian socialism and who wish to be brought up to date on an important new trend in Italian historiography.

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MARCELLO SAIJA. *Un "soldino" contro il fascismo: Istituzioni ed élites politiche nella Sicilia del 1923*, Catania: CULC. 1981. Pp. 316. L. 19,500.

Italy's transition from liberalism to fascism is usually presented in terms of developments in the northern and central regions of the country. Thus the socialist militancy, squadrist violence, political conversion of the middle classes, and merger of blackshirts with old elites that characterized the advance of fascism

in these regions have become standard ingredients in most interpretations. Yet, as Marcello Saija points out in the highly suggestive introduction to his study of certain antigovernment demonstrations in 1923, such an explanatory scheme is woefully inadequate for developments in the South, where fascism appeared as a "non-necessity." Above all, he argues that it obscures the crucial conflict between fascism and the politically dominant bloc in Sicily. As was not the case in the North, the blackshirts confronted a strong and secure political establishment there that it was able to defeat only with the massive use of state power. According to Saija, the "soldino" affair—a series of anti-Fascist protests in which demonstrators wore a coin, showing the head of the king, to emphasize their loyalty to the monarchy—represented the moment when the old notables surrendered to fascism. In its aftermath, the regime replaced the local political groups with the unitary structure of the party-state and introduced important new methods of social and political control in the South.

Three long chapters comprise the body of Saija's book. The first focuses on the province of Messina and traces the steadily escalating struggle between an ultra-Fascist prefect and the provincial political establishment headed by Duke Giovanni Antonio Colonna Di Cesarò after the march on Rome. The second chapter follows the spread of the "soldino" demonstrations from Messina to other southern cities and links them to a larger but unsuccessful project of the local political elite to prevent the penetration of fascism into Sicily. In his final chapter, Saija looks at the judicial repercussions of the affair that completed the Fascist conquest of the island. The remaining three-fifths of the book consists of documents from the state archives.

Unfortunately, Saija's introduction promises considerably more than his book is able to deliver. He does provide an extremely detailed narrative account, but only at the expense of both analysis and the larger context in which events took place. Although he offers a day-by-day account of the machinations and maneuvers of prefects, ministers, and prominent politicians, there is, for instance, no clear picture of who the local Fascists actually were. His treatment of the affair's legal aftermath has few clear connections to the themes raised earlier in the book. Nor do Saija's introductory disclaimers justify his virtually total neglect of the underlying structures that shaped and conditioned political developments. Indeed, his refusal to deal with the social and economic coordinates of political power makes it extremely difficult to evaluate his thesis concerning the politically modernizing thrust of fascism in the South. At best this is a book for the specialist already well grounded in the history of the region.

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DENIS MACK SMITH. *Mussolini*. New York: Alfred A. Knopf. 1982. Pp. xiv, 429. \$20.00.

The publication of *Mussolini* by Denis Mack Smith, Senior Research Fellow of All Souls College, Oxford, and a prolific scholar in the field of modern Italian history, is a welcome addition to the literature on the Fascist dictator. The author labels his study a "political biography" and cautions that it is not a history of Fascist Italy and still less a general history of the years between 1920 and 1945, when fascism became a dominant theme in European politics. Mack Smith is not a revisionist. In his satiric indictment of Mussolini, the author draws cleverly on the Fascist leader's own rhetoric to expose his demagogic opportunism and love of violence and war. Many of the mysteries surrounding Mussolini have become clearer since 1945, and, Mack Smith observes, the controversies are now somewhat less passionate in a political sense. Nevertheless, "there is still plenty of room for disagreement over individual episodes and differences of interpretation about someone who contradicted himself so freely" (p. xiv).

Mussolini had to fight his way out of obscurity by his own ambition and talents, the author suggests. He succeeded so well that he ruled Italy as a dictator for two decades and gained more adulation from the Italian populace than anyone else in that country's history. But at the height of his success, the Duce "fell an easy victim to the flattery that he invited or ordered from his cronies, and was beguiled into playing for the yet higher stakes of world domination" (p. xiii). For such stakes Italy lacked the material resources, and Mussolini the necessary personal qualities of mind and character. An emphasis on appearance rather than on reality always typified his style. "By the time of his death in 1945 [Mussolini] left to his successors an Italy destroyed by military defeat and civil war; he was, by his own admission, the most hated person in the country; and having once been praised to excess, was now being blamed for doing more harm to Italy than anyone had ever done before" (p. xiii).

It is difficult, Mack Smith observes, to plumb the personality of a man whose reputation moved so quickly from one extreme to the other. In spite of his bombastic posturing before the crowds, Mussolini was a very private person, the author insists. At no point did the Duce have a friend who could pass on to a later generation a balanced and persuasive assessment of his personality. Nor was there any close associate to whom he revealed himself naturally and unambiguously. In his relations with other people, Mussolini always seemed to be an actor on stage, presenting a series of parts that are not always easy to disentangle. In addition to being a gifted actor, the Duce was also a first-rate journalist and propagandist, whose public statements and private

comments were often intended to conceal the truth as much as to reveal it.

Mack Smith's new study largely supersedes previous biographies in English—namely, those by Sir Ivone Kirkpatrick, Laura Fermi, Christopher Hibbert, and Richard Collier. An exception, however, is Gaudens Megaro's pioneering study, *Mussolini in the Making* (1938; reprint ed., 1967), which sheds much light on young Mussolini's career as a socialist.

In Italian there are two major biographies. The dictator's admirers, Duilio Susmel and Giorgio Pini, brought out a four-volume *Mussolini: L'uomo e l'opera* (1953–55). Susmel and his father Edoardo have also edited a forty-four volume collection of the Duce's writings, *Opera omnia* (1951–). More recently, Renzo De Felice's multivolume *Mussolini* (1965–) draws upon a variety of archives. Five fat tomes have appeared, bringing the story to June 1940. One more volume will cover the war years. De Felice promises thereafter to compose a single-volume synthesis. All scholars are indebted to De Felice for his valuable research, but his overlong account—really a "life and times"—suffers from an almost unreadable style. And, as Mack Smith has pointed out, De Felice is sometimes insufficiently critical of Mussolini.

Certainly no one can accuse Mack Smith of unreadability. He focuses his tightly written biography sharply on Mussolini. The author is not a psychohistorian, but he has a keen eye for uncovering important facets of Mussolini's personality. He is especially good in the section where he discusses Mussolini's dream in the late 1930s of creating a new "fascist man." The author explains that this goal was little more than a projection of Mussolini's own personality. There is also a fine chapter on Mussolini as leader, and an excellent discussion of the Duce as "the private person." The passages that tell of Mussolini's repeated refusals, for reasons of vanity, to have an interpreter present with him whenever he conferred with Hitler make it abundantly clear how the Duce was at a disadvantage in these summit meetings.

The book contains seventeen chapters. The first three deal hurriedly with the pre-1919 period. Three more cover early fascism through the Matteotti crisis. Domestic aspects of the regime take up three chapters. Five are assigned to foreign policy from 1929 until 1940, and three more to World War II. The book's greatest strength lies in its analysis of the diplomatic and military aspects of Mussolini's dictatorship, a theme that the author has also treated in *Mussolini's Roman Empire* (1976). Mack Smith's scholarship is formidable. Almost every sentence has a footnote referring to several sources. Sometimes this approaches overkill. Notes and bibliography together fill almost one hundred pages.

There are some gaps. The author does not devote

enough space to economic policies: only seven pages deal fleetingly with this important dimension of Mussolini's regime. Mack Smith could also have said more about church-state relations, antisemitism, and the role of the anti-Fascists at home and abroad. American readers may feel that the dictator's interest in the United States has been slighted.

The chief weakness, however, is Mack Smith's deliberate omission of the historical background for Mussolini's actions. This is a pity, because the book is clearly intended to be read not only by scholars but also by a much wider audience. One can hardly expect the layman to be conversant with the complicated background of this period of Italian and European history. In particular, the introductory and concluding sections take too much for granted. In the latter case, the role of the king and the army in bringing down Mussolini in 1943 should have been made clearer. There is no specific mention of the Allied invasion at Salerno, of the new Committees of National Liberation, of the government in Rome of Ivanoe Bonomi (1944–45), of Pope Pius XII, or of such Resistance leaders as Ferruccio Parri and Luigi Longo. The book ends all too abruptly with Mussolini's execution by unnamed Communists.

On balance, however, Mack Smith's biography is a first-rate biography that will long be consulted for its generally well supported arguments.

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BRUCE MCGOWAN. *Economic Life in Ottoman Europe: Taxation, Trade, and the Struggle for Land, 1600–1800*. (Studies in Modern Capitalism.) New York: Cambridge University Press or Éditions de la Maison des Sciences de l'Homme, Paris. 1981. Pp. xii, 226. \$39.50.

Bruce McGowan, formerly associated with the University of Michigan and presently the director of USICA in Ljubljana, Yugoslavia, has dealt in this relatively short book with some of the fundamental problems concerning the socioeconomic transformation of Southeast Europe during the Ottoman rule from 1600 to 1800. The five chapters of the book cover the topics of Ottoman exports to Europe, land, taxation, and population and give an analysis in depth, as a case study, of the land and fiscal situation in the province of Manastir (western Macedonia—now Bitolia) from 1620 to 1830.

The book begins by describing, with a wealth of figures as well as ample references to Western sources, the relatively close commercial relations that developed between Eastern and Western Europe during the Ottoman era. The author cites in support of his views Braudel's opinion that the

economic changes in Western Europe were linked to or had a counterpart in the Ottoman state. McGowan also touches lightly on Immanuel Wallerstein's concept of global economy under which peripheral economies, pressed by growing demand from the West, increased the production of agriculture commodities through coerced labor and were eventually incorporated into the central economy in dependent relationships. McGowan thoroughly documents the already-known fact that Ottoman trade with the West in agricultural commodities increased sharply in the eighteenth and nineteenth centuries. (I believe that this trade consisted also of manufactured items, but that its volume was rather low in comparison with internal trade and that, contrary to Wallerstein's thesis, the economic relationship with Europe led to subservience to the West only in the nineteenth century when political means were used.) McGowan agrees that the sector of Ottoman agriculture aimed at export grew very slowly in the seventeenth and eighteenth centuries.

The main, and most original, part of McGowan's book deals with land and people or, to be more specific, with taxation and the formation of *chiftliks*, the large land estates in Ottoman Europe. Balkan historians have studied extensively—especially since World War II—the breakdown of the Ottoman classical land tenure system and the passage of the land to various individuals who supposedly used it to produce agricultural commodities for profit. This research by Balkan Marxist scholars was intended to prove the existence of the classical sequence of feudalism, capitalism, and (eventually) socialism—hence the universality of the event—even in lands such as the Ottoman empire with different sociocultural systems. The effort produced a number of excellent studies, along with one-sided works in which sweeping generalizations based on isolated data mix freely with predetermined conclusions and jingoistic laments blaming the Turks for all the imaginary and real woes of the Balkans. As a whole, these studies tend to agree that after the sixteenth century the lands of the eminent domain passed into the hands of private individuals. McGowan, on the other hand, through an exhaustive and unique study of the Ottoman tax registers and *sicil* records (decisions of the local courts) of Manastir province, proves: that while some land was usurped by individuals, mostly former *timar* holders or officials, the number of *chiftliks* remained more or less constant in the seventeenth and eighteenth centuries; that, on average, such estates were relatively small in size; and that they constituted a rather small percentage of the total cultivated area. McGowan believes that the rise of the *chiftlik* was not the result of market pressure but of the “shifts in fiscal and administrative practices intended as compensating devices to

deal with the shrinking tax base" (p. 121). I fully agree with this statement, not only because of the author's overwhelming original supporting data and exhaustive evaluation of the published material in Turkish, Serbo-Croatian, Bulgarian, and Western languages, but also because his conclusions mesh logically with the socioeconomic and political transformation of the Ottoman realm in the nineteenth and twentieth centuries.

McGowan's study of the Ottoman population is based on already overutilized published sources and is rather unsatisfactory: for example, a study of the original *defter* containing the results of the census of 1831 would have given him better insight into the number of non-Muslims in the Balkans and of those paying and not paying the *tiziye* (*ciziye* or head tax).

There are a few other minor problems of nomenclature and definition, but as a whole this is a careful work showing a high quality of scholarship, erudition, objectivity, and balanced judgment. It is by far the best book on Southeast Europe published in English in the last twenty years, and it ought to be read by all Ottomanists and, especially, the Balkanists.

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CONSTANTIN G. PATELOS. *Vatican I et les évêques uniates: Une étape éclairante de la politique romaine à l'égard des orientaux, 1867-1870*. (Bibliothèque de la Revue d'Histoire Ecclésiastique, number 65.) Louvain-la-Neuve: Collège Érasme or Éditions Nauwelaerts, Louvain. 1981. Pp. xv, 567.

Constantin G. Patelos sounds his theses early and they echo throughout his work. The First Vatican Council (1869-70) represented the culmination of the Tridentine thrust toward a strictly centralized papal monarchy. Only the Church of Rome was fully *Church*. Thus the Uniate communions, gathered unto Rome by Latin missionaries toiling among the Eastern Christians, particularly in the Ottoman empire, were deemed deficient to the degree that they remained unlatinized. Ultramontanism, spawned by Trent and nurtured by Rome's hostility to nineteenth-century ideologies, compromised the original Uniate rationale whereby Eastern Christendom would be "restored" to papal obedience while retaining its ancient rites. Even before Vatican I's stark pronouncements on papal primacy and infallibility, the Propaganda's advocacy of latinization, reinforced by the encyclical *In Supreme Petri* (1848) and the bull *Reversurus* (1867), had virtually redefined "rite" as mere liturgy. Rite as the complex amalgam of liturgy, canon law, custom, and an Eastern ecclesiology that centered about the auto-

cephalous patriarchate yielded to the ultramontane imperatives. Latin-Uniate relations became strained while hope for Orthodox-Catholic rapprochement dissolved. Uniate inability to inhibit this evolution, despite the efforts of a few prelates, was a consequence of Latin preponderance, Uniate disunity, and the latinized education of many Uniates. Nor was the opposition of France, inspired by its concern that papal centralization would undermine its "protectorate" over Catholic Christians in the Ottoman empire, any more effective.

Patelos's contentions are largely correct, although he probably underestimates the extent to which the Uniates fostered their own latinization. For many Christians of the Middle East (the author's central focus) entry into the Unia was a step toward Westernization, toward identification with the civilization busily establishing its global hegemony. Patelos seems unaware that in the eighteenth century only Roman intervention kept many Uniates from plunging into the Latin rite. Latinization, like the European imperialism to which it was organically related, tended to be most attractive to the elite elements of marginal communities such as the Arabic-speaking Christians. Perhaps the tragedy of the traditionalist Uniate prelates of Vatican I lay less in being arrogantly dismissed by Latins than in their inability, *as Uniates*, to admit that the prerogatives they defended could obtain fully only within the conciliar church of Orthodox ecclesiology, not within the Tridentine church to which they had bound themselves.

Patelos's study has not made the transition from dissertation to book with perfect grace. It is repetitious. One-third of the text comprises biographical notices of prelates, few of whom figure prominently in the narrative. Some of Patelos's theses have been voiced before, particularly by his mentor R. Aubert and by Joseph Hajjar, the Melkite scholar omnipresent in the footnotes. A study of Rome's relations with seven Uniate churches in the era of Vatican I should ideally be anchored in the vast documentation available. While linguistic demands stand between most scholars and full control over the extant documentation, Patelos's acknowledgment of lacunae in his research does not make his work less tentative. Withal, he has exploited with skill and sensitivity much of the sound secondary literature as well as the primary sources to which he enjoyed ready access, notably those of the Preparatory Commission for the Eastern Churches and the public meetings of the Council itself. The result is a work both serious and informative.

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GIÖRGOS DERTILES. *To zētēma tōn trapezōn, 1871-1873: Oikonomikē kai politikē diamachē stēn Ellada tou 19ou*

aiōna [The Bank Question, 1871–73: Economic and Political Controversy in Nineteenth-Century Greece]. Foreword by K. TH. DEMARAS. (Meletes Oikonomikeēs Istorias.) Athens: Morfotiko Idryma Ethnikēs Trapezēs. 1980. Pp. xx, 353.

In 1870, some forty years after attaining independent statehood, Greece remained a small, poor country. With a population of under two million, scanty resources, primitive agricultural methods (farmers still labored with the wooden plough of Hesiod's time), and hardly any industry, it is little wonder that foreign capital stayed away from Greece. The year 1871 marked the appearance of financial proposals made by wealthy Greeks of the diaspora, which held out hope of substantial foreign investment. Evangelos Valtatzis and Andreas Syngros, financiers from Constantinople, submitted competing proposals and began negotiations with the National Bank of Greece aimed at establishing a new bank in Greece. While Giōrgos Dertiles traces the course of these negotiations in detail until the founding of the new bank—the *Genike Pistotike*—in November 1872, he deals deftly with broader subjects as well: the economic ideology of the time, the impact of the Greek capitalists from abroad on the political scene in Athens, and the nature of the Greek political process.

It was, Dertiles believes, fear of an uncertain economic future in Europe as well as the Levant that prompted wealthy Greeks to move to Greece; for precisely because Greece was poorer, there was less competition there, and capital, carefully invested, could provide a potentially much more secure return. Athenian newspapers, not surprisingly, welcomed the financiers. After all, this was the century when the patriotic, irredentist, *Megale Idea* swept all before it. The capitalists from abroad, it was generally conceded, were "comparable to the heroes of the Greek War of Independence" (p. 46). Greeks at this time were also much impressed by everything French as well as by the prestige associated with industrialization. Hence the financiers' artful claim that the *Genike Pistotike* was "modelled" on the *Crédit Mobilier* (though they had not the slightest intention to help Greek industry).

Andreas Syngros, a shrewd wheeler-dealer who quickly outfoxed not only Valtatzis but also the tenacious National Bank (which did not want any competitor, least of all one with which it would have to share its lucrative note-issuing privilege), knew that to succeed he needed friends in high places. Nobody was placed any higher, of course, than George I, King of the Hellenes, whose own ambition was to expand the power of the throne. Dertiles suggests that Syngros and the other financiers represented a group the king found useful. To a German who observed that Greece lacked an aris-

ocracy, the king replied that he could rely on a "plutocracy," there being "over twenty millionaires" in Athens already (p. 135). Quite appropriately, it was Deligiorgis, whom the king appointed prime minister in July 1872 after forcing Voulgaris to step down, and it was Deligiorgis who gave final, formal approval to the deal with the Syngros. The agreement signed in November 1872 provided for the new bank to have the crucial note-issuing authority. Caught up in a veritable "bank fever," thousands of Greeks lined up a few days later to buy out all the *Pistotike* shares available. The narrative ends rather abruptly at this point, and this is the book's only defect.

Dertiles' documentation is exhaustive. Half the book is devoted to notes, well-chosen appendixes, and a bibliography. He has used Greek, English, and French archival sources, supplemented by memoirs and contemporary press accounts. To sum up, Dertiles' monograph, besides its obvious value for banking history, presents a lucid, coherent interpretation of the parliamentary changes of July 1872 that led, two years later, to the most serious political crisis of King George's fifty-year reign.

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MIRKO VALENTIĆ. *Vojna krajina i pitanje njezina sjedinjenja s Hrvatskom, 1849–1881* [The Military Frontier and the Question of Its Unification with Croatia, 1849–1881]. (Sveučilište u Zagrebu, Centar za Povijesne Znanosti, Odjel za Hrvatsku Povijest, Monografije, number 12.) Zagreb: Institut za Historiju Radničkog Pokreta Hrvatske and Školska Knjiga. 1981. Pp. 360.

The Military Frontier furnished the Habsburgs at least 15 percent of their soldiers well into the nineteenth century. Of its one million inhabitants, enrolled in fourteen regiments, one person in twelve was always on active or reserve duty. Numerous Habsburg officers, notably Joseph Jelačić, rose from the ranks of the frontiersmen. Although economically backward, the frontier supplied as many recruits as Bohemia, whose population was five times greater. No wonder the Viennese court clung tenaciously to this manpower reservoir during the revolution of 1848–49 and the foreign crises of the 1860s.

Mirko Valentić does not repeat this military, strategic, or institutional history of the frontier that is covered well by other scholars like Gunther E. Rothenberg. He focuses on the prolonged struggle waged by Croatian intellectual and political leaders after 1848 to dissolve the frontier and unite it with civil Croatia. By itself, Croatia was a rump kingdom numbering only 845,000 inhabitants according to

the 1857 census (Dalmatia remained a separate crownland). Adding the frontier would have doubled Croatia's size and population and would have laid the foundation for further national progress.

Valentić chooses a traditional interpretation and omits several relevant themes. He does not discuss, for example, the role of religious communities and leaders like Bishop Strossmayer. He does not examine in great detail the social and economic fabric of the lives of the frontiersmen or their families. He skirts the issue of demographic and cultural tension between Croats and Serbs in the frontier. Croats formed only a 60 percent majority of the population in the eleven regiments that composed the Croatian-Slavonian frontier, which stretched from Senj to Zemun; in four regiments they were a minority. Certainly the Principality of Serbia had influence among many frontiersmen, yet Valentić barely mentions Serbia in his study.

Having done extensive research in Austrian and Croatian (though not Hungarian) archives, Valentić details the tortured negotiations that led through the *Ausgleich* (1867) and the *Nagodba* (1868) to the final dissolution of the frontier in 1881. The Hungarians were nearly always the dominant party over the Croats in this process. They wanted to dissolve the frontier in order to end Vienna's control over them and exploit its economic potential by building railroads and harvesting lumber. By agreeing to increase their contribution to the monarchy's common expenses, the Hungarians obtained de facto control over the frontier in 1871. It was joined to Croatia a decade later only after Budapest had imposed heavy political conditions on Zagreb. Rather than a cause for celebration by patriots, therefore, the frontier's unification with Croatia illustrated the satellite role assigned to Croatia within the Austro-Hungarian dualist system after 1867.

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ANDREW C. JANOS. *The Politics of Backwardness in Hungary, 1825–1945*. Princeton: Princeton University Press. 1982. Pp. xxxvi, 370. Cloth \$27.50, paper \$12.50.

According to the author, the principal purpose of this study is to test and elaborate further the validity of recent neo-Marxist theories about the politics of modernization and development (p. xxi). Andrew C. Janos, a political scientist at Berkeley, was conceptually influenced by works like *The Modern World System* by Immanuel Wallerstein (1974); *Readings in U.S. Imperialism*, edited by K. T. Faun and Donald C. Hodges (1971); *Dependency and Development in Latin America* by Fernando H. Cando and Enzo Faletto (1979); and *Unequal Exchange: A Study of Imperialism of Trade* by Arghiri Emmanuel (1972). He sees the

world divided between wealthy and industrialized "core" nations in the Occident and economically underdeveloped countries in the peripheries of the world economic system. The poor, developing countries are of course exploited by the rich, and their politics are determined by their economic backwardness.

Although one could argue that oil-producing developing countries at times exploit the Occident, we should only try to assess how Janos's conceptual framework helps to explain Hungarian politics. The essence of his argument is that the great liberal and democratic ideas and institutions of the nineteenth century, which were transplanted from the West, did not lead to political democracy and a capitalist market economy in Hungary because they were superimposed on a society with an undifferentiated economic structure. Instead, between 1849 and 1906 they resulted "in a bureaucratic polity, a pseudo-market, and neo-corporatist society" (p. 92), where power was divided between politicized bureaucrats, neotraditional landowners, and ethnic—mostly Jewish—entrepreneurs. The period between 1906 and 1919 is characterized as "the revolution of the left." Afterward, the author sees "restoration of neo-corporatism" under Prime Minister Bethlen and a "revolution on the right" between 1932 and 1945.

The most successful parts of the book deal with the creation and functioning of the political machines of Premiers Tisza and Bethlen. The political and economic role of different social classes is also described and analyzed with skill and sophistication. Particularly interesting are the author's insights about the Jewish entrepreneurial element and its relationship with the "historical" classes. Janos makes a conscious effort through the entire work to present both positive and negative aspects of Hungarian politics. Although highly critical of Hungarian conditions, he also shows that in the dualist period the parliament was responsive to public opinion to a considerable degree, the press was free, and the courts were independent. Similarly, he stresses that despite the rightist regimes in the 1940s Hungary was the only country in East Europe with a functioning multiparty system and some freedom of assembly and of the press (p. 306). The book is also strong on economics and is replete with statistical tables, figures for gross national product, and comparative growth-rate indicators. Often Janos not only summarizes existing data but by combining different sources creatively is able to arrive at interesting and original insights. At other times, however, his work shows lack of familiarity with recent monographic literature on Hungarian history and is predominantly influenced by older general works. There are also some rather disturbing factual errors. He appears to believe, for instance, that the

official language in Hungary changed from Latin to Hungarian in 1841, that the deputies at the Pressburg diets paid for their quarters, and that Prime Minister Schwarzenberg died in 1854 (pp. 11, 40, 88). The author is also guilty of occasional inexact use of his sources (p. 54, n. 59; p. 93, n. 14).

But the greatest shortcoming of the book is related to the author's underlying assumption that a backward economy automatically produces politics of backwardness. In Janos's view, before 1848 backwardness transformed the laissez-faire doctrine of liberalism in Hungary into protectionism. The electoral manipulations of the dualist era, the occasional need of royal threat to secure the cooperation of the Upper House ("pairschub"), and the victory of right-wing radicalism in the 1930s and 1940s are all presented as manifestations of the politics of backwardness. The author does not seem to be aware that all these "negative" features of Hungarian politics were also part of the English and French political experience, and he strangely disregards the fact that right-wing radicalism before World War II was a general European phenomenon.

Thus, evidence that does not fit into the conceptual framework tends to be neglected. For example, nationalism, certainly a major motivating force in Hungary, receives minimal attention, and the 1848–49 war for freedom (hardly a sign of backwardness) was dealt with only in one paragraph (p. 87). Although Janos is aware of Hungary's ties to the West, he still uses the analogy of the Third World countries to explicate Hungarian conditions. Among others, he bravely claims at the end of his book that the "capabilities and limitations" of Hungarian right-wing Premiers Gömbös, Imrédy, and Sztójay, have been the same as those of Nasser, Nkrumah, or Amin, and compares the cases of Faruk versus Nasser, Idrin versus Khadaffi, and the shah versus the ayatollah to the struggles of Premiers Tisza and Bethlen with their own opposition (pp. 318–19).

These ideas, interesting in themselves, could be accepted only on the basis of detailed and thorough comparative analysis, which the author does not provide. He does call attention to the importance of backwardness in Hungarian politics in a creative and imaginative fashion. But it seems to this writer that so far no one has succeeded in explaining a country's politics adequately from a single cause. Therefore, we should continue to see the mutual interaction of numerous historical forces in Hungary, which indeed was a "developing country" between 1825 and 1945, but whose place in world history is among the borderlands of *Western* civilization.

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GARY B. COHEN. *The Politics of Ethnic Survival: Germans in Prague, 1861–1914*. Princeton: Princeton University Press. 1981. Pp. xvii, 344. \$27.50.

The history of urban politics in the Habsburg monarchy in the nineteenth century frequently offers microcosmic portraits of changes in the distribution of class and cultural resources within the networks of organized political authority with broader regional and national implications. Local history is thus more than simply "local" in importance. This was certainly the case in Vienna and in Graz; nowhere was it more apparent than in Prague, where a small, embattled German community not only found itself surrounded by an increasingly militant Czech majority but also found itself divided in its own internal responses to the challenge represented by that majority.

In this new study of the cultural and political isolation of the German community in Prague between 1860 and 1914, Gary B. Cohen offers a penetrating analysis of the evolution of the conflict between Czechs and Germans in the Bohemian capital. Cohen skillfully weaves a detailed analysis of demographic and class-status materials involving the German community (both gentiles and Jews) into a larger consideration of the political behavior of three generations of German voters in Prague. This should be emphasized, for Cohen's book is, for all of its utilization of modern social historical methods, essentially a political and organizational-associational history of the conflict between Czechs and Germans in Bohemia. Personally I found this approach salutary, since it offers a dynamic view of the changing consciousness of the German community over time—its public and private expression of social hierarchy, familial values, economic goals, and religious and ethnic identity—rather than a static portrait of isolated demographic trends, such as has become in recent years the staple product of much East Central European social history. Cohen has a dramatic story to tell, one that involves the collective psychological history of a fearful, isolated group of German-speaking inhabitants in Prague. The book thus addresses larger questions involving the distribution and use of political power and the collective articulation of cultural self-identity, moving beyond a social history of quantitative data to confront the historical actors themselves in all of their subjectivity, hysteria, and prejudice.

Cohen begins his analysis with a survey of Czech-German relations before 1860, moving to a description of the emerging national-ethnic consciousness of the German community in Prague as a result of the economic and cultural challenges posed by the Czech majority after 1860–65. A third chapter offers a detailed demographic portrait of the German community between 1870 and 1900, and the

final three chapters of the book survey attempts within the community at associational and political defenses against the Czech threat and the gradual fragmentation of the Germans' own public political unity as new mass-based issues and more socially differentiated participants entered the public scene in the 1880s and 1890s.

Of importance in this book is Cohen's conclusion that political rubrics are essentially malleable and fluid in their utility and efficacy and that precise social lines between various intra-German political factions are difficult to draw with absolute certitude. Cohen rightly notes, for example, that each faction of the German community drew members from a variety of social strata (although he does see predominant groups represented in each) and that change within the community was as much intra- as intergenerational. Antiliberalism was not merely a revolt of fathers against sons, but also a revolt of many fathers *and* sons against previously sanctimonious political values shared by both.

A major analytic assumption of the book is that subjective ethnic identity is more than an analytic *deus ex machina* and that it can be correlated, if only in very cautious and tentative ways, with class and status rankings. Cohen sees the growth of German subjective national consciousness as a direct reflection of Czech challenges to German economic and status hegemony in Prague after 1860. German nationalist and German liberal elites were divided by their relative location at different ends of the same *bürgerlich* spectrum of opinion and status (with a good deal of overlapping between the two). As factionalized as these intra-ethnic groups were, both were dramatically separated from the masses of Czech working-class and lower middle-class inhabitants of the city. Although this assumption may provoke some discussion, it does have the merit of allowing the historian to posit a multi-causal framework of motivation and action in explaining the origins of national identity. But it also re-opens the perennial Austrian administrative problem of why ethnic hatred, which reflected (among other factors) an imbalance in social and economic resources, was not reversible along with such imbalances. Material inequality and elite defensiveness may explain the origins of subjective national consciousness, but they do not explain its persistence under circumstances that might have allowed both groups to coexist on relative status-class equality. Cohen is, of course, aware of this issue, and the emphasis that he places on nonmaterial ideological and psychological features of the problem offers a more balanced approach.

The book also contains a good discussion of the effect of the new interest-group politics on traditional political institutions, although Cohen stresses slightly more than I would be inclined to do the

transformative effect of such interest politics. (One might argue, for example, that the Austrian Christian Socials did not, in their elite views, differ very much from the sentiments expressed by the German Liberal Karl Eppinger (p. 257) on the need for supra-interest-based, nonclass parties in which political authority maintained independence from and control over interest articulation.) In his description of the behavior of such groups (as opposed to the extent to which they actually succeeded in fragmenting the political system), Cohen is clearly correct. The case of the various state officials' groups of all nationalities after 1907 revealed an almost unlimited level of egoistic partisanship that often exploited the nationality issue for deeper socioeconomic advantages. The general director of Austrian Post and Telegraphic Affairs, Friedrich Wagner v. Jauregg, wrote to the German National leader Gustav Gross in late 1914 arguing that in his professional experience much of what passed for national antagonism among state officials of various ranks was merely a conscious ploy to obtain material advantages on an individual or group basis with little regard for ethnicity as a value (see HHSA, *Nachlass Gustav Gross*, Karton 2, letter of November 24, 1914). Such views support the contention of Cohen's book that future research must utilize a multimotivational framework to explain national antagonisms.

Inevitably, the scope of such a book necessitated choices between what was covered in depth and what received merely cursory treatment. The book might have profited from a greater elaboration of the public institutional framework of urban government and regional politics in which the communal social and cultural conflicts occurred. The role of *Reichsdeutsch* economic, academic, and diplomatic interests and the pressure brought to bear on Vienna by the German government during the various periods of Austrian "uncertainty" about how to deal with the Czech-German problem in Prague (and elsewhere in Bohemia) might have offered an opportunity to anchor this story in a larger context of state policy toward nationalities. These are valid subjects for additional research. The book does contain, moreover, many suggestions for future comparative work on ethnic politics within the monarchy, especially the situation in Moravia.

Cohen's study is an authoritative analysis of this subject, with which all future historians of Czech social and political history will have to come to terms. His judicious use of social-historical materials, especially his careful sampling of census data, and his thorough reading in all aspects of Bohemian political and social history combine to make this a monographic study of larger value for historians of Central and East Central European political and intellectual history, including specialists interested in East European Jewry. The book will also be of value

to historians of comparative urban social history, and to those interested in nationalism and its role in the mobilization of Imperial German politics after 1885. Finally, this book offers a model for the dispassionate analysis of the most tumultuous political issue of the old empire—the hate-ridden struggle between Czechs and Austro-Germans from 1860 to 1918 (and beyond). This struggle was characterized by fear and anxiety on the one hand and by arrogance, stupidity, and selfishness on the other, aspects of behavior shared in full measure by the protagonists of *both* camps, from which only losers emerged in the long run. Let us hope that future historiography of the relationship between Czechs and Germans elsewhere in Bohemia and in Moravia maintains the standard of fairness and impartiality set by Cohen.

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NORMAN DAVIES. *God's Playground: A History of Poland*. Volume 1, *The Origins to 1795*; volume 2, *1795 to the Present*. New York: Columbia University Press. 1982. Pp. xxxiii, 605; xxvii, 725. \$35.00 each; \$60.00 the set.

This is a remarkable book. Norman Davies, a British historian from the London School of Slavonic and East European Studies, singlehandedly undertook the formidable task of writing a comprehensive Polish history, a feat not attempted by an English writer for nearly a century and achieved by few other scholars. While Davies modestly describes his two-volume work as a survey, it is clearly more than that. Seeking to go beyond political history, Davies tries to encompass in his work social, economic, cultural, and ideological themes weaving a complex picture of panoramic dimensions. In order to achieve this effect, the author draws extensively, in addition to the more usual historical sources, on diaries, travel guides, chronicles, belles-lettres, and even songs. The organization of the two volumes is partly topical, partly chronological: thematic chapters followed by narrative sections. Finally, this is a very personal book, which reflects Davies's philosophy of history and his interpretation, often forcibly expressed, of Poland's past and present.

Norman Davies is a product of two very distinct historical schools, that of Oxford where the brilliant, erratic, and controversial A. J. P. Taylor had been his mentor, and of the postwar Jagiellonian University where he was exposed to deterministic and "causalistic" Polish historiography heavily tinged by "Marxism." He himself holds the view that "the matrix of historical forces determines the limits of the possible, but does not, and cannot determine what course events will subsequently take within those given limits." As for human motivation he

believes "in the primacy of the irrational, seeing Reason as the servant, not as the master of our fears, emotions, and instincts" (p. viii). If one adds the author's penchant, whether derived from Taylor or not, for the paradoxical and the unorthodox, one can readily see why this book differs from most other works on Polish history.

The work consists of three distinct parts. The first surveys medieval and early modern history in two thematic chapters on historiography and historical geography and three chronological chapters that bring the story up to 1572. Thus, *God's Playground: A History of Poland* does not cover adequately the nation's past from the origins to the present, for the first six hundred years are dealt with in less than one hundred and sixty pages out of the total of some thirteen hundred. The title "Introduction" affixed to this part makes that clear. The work is then essentially a history of the last four centuries.

The most interesting second part, entitled "The Life and Death of the Polish-Lithuanian Republic 1569–1795" occupies the rest of the first volume. Chapters 6 to 11 are topical, and the respective themes are: "Religion," "Society," "Economy," "the Cities," "Constitution," and "Foreign Affairs." They are followed by chronological chapters with such titles as "Valois," "Bathory," "Vasa," "Michał," "Sobieski," "Wettin," and "Agonia." The volume ends with the partitions of Poland.

The third part, which corresponds to the second volume, covers the period from 1795 to the present (meaning roughly the year 1976). The same division into thematic and chronological chapters prevails. To the former category belong: "The Growth of the Modern Polish Nation," "The Russian Partition," "The Prussian Partition," "The Austrian Partition," "The Process of Industrialization," "The Rise of the Common People," "The Roman Catholic Church," "Education and the Cultural Heritage," "The Jewish Community," "The Military Tradition," and "The Polish Emigration." To the latter, "The Duchy of Warsaw," "The Congress Kingdom," "The Republic of Cracow," "The Springtime of Other Nations," "The Thaw and the January Rising," "Revolution and Reaction," "The Rebirth of the Polish State," "Twenty Years of Independence," "Poland in the Second World War," "The Modern Polish Frontiers," and finally the last two chapters, set apart by a subdivision, "The Communist Movement," and "The Polish People's Republic." The "Postscript" is really a short essay ruminating on Poland's past, present, and future, and a six-page addition called "Solidarity" is a last-minute endeavor to bring the volume up-to-date and stress this important event in contemporary Polish history.

The book is lavishly supplied with maps, diagrams, and photographs, most of which are very useful. Bibliographic aids are less helpful. Footnotes

to the individual chapters do not always reflect the wealth of existing sources and literature; "Suggestions for Further Reading" are unfortunately no substitute for a good bibliography. The absence of the latter is apparently not the author's fault but results from economies on the part of the publisher, which can only be regretted.

A short review cannot do justice to a work of such scope, and only some points can be made here. The imaginative organization of the book has its pitfalls, for the thematic chapters do not sufficiently differentiate between periods, and the chronological sections, not to be repetitive, tend to be too brief. Neither are fully comprehensive, creating a problem for the student who will not wish to read the entire work. The cohesiveness of a book written by a single author is achieved at a price of a certain lack of expertise—one can hardly be an expert on all the subjects discussed, and Davies is no exception. Some chapters are clearly better than others. There are omissions and unavoidable slips and mistakes. Moreover, Davies tends to take a strong stand on a number of issues, and some of his opinions are highly controversial. His stress on discontinuity rather than continuity of Poland's past is open to question. His bold statements, as for instance on the recreation of Poland in 1918 (p. 392) or on independence being an exceptional state in Polish modern history may be resented or misconstrued. A reader, depending on the degree of his sophistication, may draw various inferences from this book that may surprise the author. My own reactions to *God's Playground* ranged from admiration for Davies's erudition and originality to occasional annoyance with some of his more sweeping assertions. But, whatever the reservations, one cannot deny that this is a major work that is imaginative, thought-provoking and extremely well written. No serious student of Polish history can ignore it.

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MAREK ANDRZEJEWSKI. *Socjaldemokratyczna partia wolnego miasta Gdańska, 1920–1936* [The Social Democratic Party in the Free City of Gdansk, 1920–36]. Summaries in German and Russian. (Seria Monografii, number 74.) Gdańsk: Gdańskie Towarzystwo Naukowe or Ossolineum, Wrocław. 1980. Pp. 243. 45 Zł.

Typical of Polish studies of "party history," Marek Andrzejewski's analysis of the Social Democratic party in Gdańsk (SDPGd) is descriptive rather than analytical and chronological rather than topical. Five major themes emerge from this study: internal party politics, SDPGd and Gdańsk politics, the relationship between SDPGd and the German political

scene, connections between SDPGd and the German Social Democratic party, and the relationship between SDPGd and Poland.

Andrzejewski describes the public face of SDPGd but tells us little about internal party politics. He summarizes the weaknesses of the party—passivity, bureaucratization, lack of ideas, legalism, impoverishment, and political naiveté. The author does not, however, depict internal party politics with any real clarity. Leaders have names (sometimes), changes occur in the leadership ranks, and policies change without clearly defined motivation. For example, after the Nazis came to power in Gdańsk, new men emerged in SDPGd, replacing leaders who had directed the party from the foundation of the free city. We know the names of the new leaders and that they represent a less nationalistic and a "more radical" position, but we learn little about the situation within the party that produced these potentially important changes. Providing us with a vague and dissatisfying portrait of internal politics within SDPGd, the author devotes much of the book to SDPGd relations with the "external world."

Andrzejewski addresses himself to the party's relationship with Germany, the German SDP, and the Poles. He portrays Gdańsk as a microcosm of Germany, with political changes in Germany almost automatically producing parallel changes in Gdańsk. The author shows that SDPGd tied its fate to that of the SDP in Germany; yet, subordination gave the Gdańsk party little political or financial help. Andrzejewski allows an ideological perspective—a "progressive party" should be pro-Polish—to determine his analysis of the relationship between SDPGd and the Poles. He asserts that the actions of SDPGd were pro-Polish and beneficial to Poland. The details he presents show that for SDPGd nationalism was more important than socialism (Gdańsk should be returned to Germany); that SDPGd realized that electoral support would decline if it was perceived as being willing to make concessions to Poland; and that, when SDPGd favored an accommodation with the Poles, it was always with the ultimate goal of benefiting the Germans in Gdańsk.

The book is not clearly focused: it includes long asides on the Gdańsk Nazi party and a lengthy justification of Polish foreign policy toward the German Nazis. It is unfortunate that the study concentrates on just the "public" SDPGd. Finally, it adds little to our knowledge of politics in Gdańsk in the interwar period.

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BRENDA MEEHAN-WATERS. *Autocracy and Aristocracy: The Russian Service Elite of 1730*. New Brunswick,

N.J.: Rutgers University Press. 1982. Pp. xii, 274. \$25.00.

Was there an aristocracy in eighteenth-century Russia? The presence or absence of an aristocracy is a prime consideration in interpreting not only the European-wide issues of the social bases of absolutism and the devolution of power in the *ancien régime* but also, in the exclusively Russian context, the policies of westernization sponsored by Tsar Peter I (1682–1725). Brenda Meehan-Waters addresses a classic historiographical issue in her study of Peter's reform of the hereditary elite of seventeenth-century Muscovy, the boyar families, into the service elite of imperial Russia.

Meehan-Waters measures the effects of the reforms by tracing the career patterns, social origins, wealth, kinship, and clientele links of the 179 members of the *generalitet* (the holders of the top four bureaucratic ranks on the Table of Ranks) in the earliest available list of 1730. By computerizing the data and sifting its correlations carefully, the author argues that the Petrine reforms were designed neither to replace the traditional, Muscovite aristocracy with a meritocracy of "new men" posted on the basis of talent and demonstrated expertise nor to extend to foreign military experts the political influence traditionally reserved for the boyars. Rather, Peter's policy was a "blend of coercion and preferment" with the goal of recoding the values and retraining the skills of the Muscovite aristocrats for services appropriate to the imperial state. The success of his efforts was proven by, for example, the functional continuity in a serviceman's status and office between the Muscovite and imperial periods; that is, boyars of the seventeenth century begat senators in the eighteenth century, or presiding officers in the Muscovite administrative chancelleries begat presidents of the Petrine colleges (p. 35). This and other particulars carry the general conclusion that the *generalitet* of 1730 was dominated by the old Muscovite families whose lineage gave luster and legitimacy to the new Petrine state and whose service provided justification for positions of privilege.

There are clear-cut limits to defining the characteristics of a modernized aristocracy. As the author mentions, the meritocracy of rank did not deny the privilege of the wellborn, that is, one could be ennobled by service achievement but could not lose inherited nobiliary status by the lack of service. The formal procedures for promotion in rank for proven merit were paralleled by traditional patron-client relations, a system whose bases were devoid of considerations of principle and merit. Most importantly, the autocracy suffered no loss of power to a modernized aristocracy. The state's discretionary

power allowed it to reward its servitors with land or to punish them by confiscation of their estates (p. 3). If indeed there was an aristocracy, it was one that neither had to be modernized nor had to suffer any loss in the benefit of familial connections. It did not have the resources to counter the state's power to make or break an aristocrat at will.

The Russian aristocracy of the early eighteenth century was not in any way comparable to its European counterparts, particularly Prussian Junkerdom. Although the imperial aristocracy surely did not constitute a state within a state (or even an estate with legally guaranteed corporate privileges), it was a politically astute and mature grouping of prestigious families in the upper nobility, one whose ranks were only marginally changed from generation to generation. Without need to equate political maturity with political opposition, analysis suggests that a reforming autocracy and a modernized aristocracy were engaged in the process of a political compromise of interests—leading toward the eventual triumph of neither one. Reconciliation and stability were the goals assumed by both parties. Meehan-Waters has written a fresh and challenging interpretation of elite politics in eighteenth-century Russia. Her book is a major work on a major topic with major significance for the study of not only the imperial aristocracy but also the elite in *ancien régime* Europe.

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L. E. SHEPELEV. *Tsarizm i burzhuaizma vo vtoroi polovine XIX veka: Problemy torgovo-promyshlennoi politiki* [Tsarism and the Bourgeoisie in the Second Half of the Nineteenth Century: Problems of Commercial-Industrial Politics]. Leningrad: Nauka. 1981. Pp. 274. 2 r. 20 k.

The main title of this work is somewhat misleading, fortunately. Rather than some ponderous and no doubt largely derivative Marxian analysis of the relationship between the tsarist government and some all-too-unified middle class, this valuable study is sharply focused and combines the strengths of the monograph and the intermediary synthesis. Building upon an earlier monograph on joint-stock companies in Russia and government policy on corporations, L. E. Shepelev has analyzed the evolution of general economic policy as formulated by the Ministry of Finance from 1856 to 1904. Since scholars have long recognized that the Ministry of Finance led all government efforts to promote industrialization and economic modernization throughout this

long period, Shepelev's detailed investigation has many implications for students of both Russian economic development and the bureaucracy.

While probing the infrequent general policy statements of successive finance ministers, Shepelev effectively unifies his work by concentrating on the ministry's recurring problems and daily concerns. These included taxation and budget deficits, government regulation of business, monetary matters and the gold standard, tariff protection, foreign capital, the labor question, and schemes to improve the coordination of economic policy. All these matters are carefully examined, and many specialists will find fresh and useful information. I was particularly struck by Shepelev's demonstration of the powerful, generally overriding influence of budgetary deficits on the formulation of policy until the late 1880s. Equally striking was the related conviction of all finance ministers, from M. K. Reutern through Sergei Witte, that the Ministry of Finance desperately needed to attract foreign capital and then defend it against domestic foes. The ministry's long and well-known conflict with the Ministry of Internal Affairs, especially on labor questions, is also effectively analyzed.

One appealing aspect of this work is Shepelev's keen interest in the personalities of the ministers and their coworkers. No less refreshing is the attention to corridor politics and bureaucratic power plays. The actors come alive and they matter. Most valuable of all for this reviewer is the wealth of archival material quoted and summarized. Here Shepelev carries forward the tradition of the Lenin-grad branch of the Institute of History, which has developed a flair for mining the rich ore at their disposal. Western scholars who have taken a turn in those mines cannot help but be appreciative.

Two of Shepelev's general interpretations are especially noteworthy. First, stressing the continuity of the Ministry of Finance's basic positions, Shepelev concludes that the ministry's numerous and powerful agrarian and conservative opponents were nonetheless incapable of reversing or derailing the main thrust of commercial and industrial policy. Second, buttressing his Marxian argument with unusual attention to Friedrich Engels's famous letters to N. F. Danielson, Shepelev stresses the underlying similarity of Western and Russian capitalist development, thereby diverging markedly from American scholarship's delight in emphasizing differences. Shepelev's conclusion might be more convincing if he (and other Soviet scholars) paid closer attention to private entrepreneurs and their critical role in capitalist economic development.

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NEAR EAST

BERNARD LEWIS. *The Muslim Discovery of Europe*. New York: W. W. Norton. 1982. Pp. 350. \$19.95.

The theme of this book as announced in its second sentence is the tracing of a Muslim discovery of Western Europe "parallel" to the discovery of the rest of the world by Western Europeans from the fifteenth century onward. Yet the substance of the work actually constitutes a demonstration that no such parallel discovery occurred prior to the nineteenth century, a period that is specifically treated only in the last five pages. Muslim curiosity about Western Europe during the preceding millennium is persuasively shown to have been virtually nil and mainly utilitarian.

In the course of the exposition the reader is amused and informed by a wealth of anecdote and direct quotation drawn from Lewis's long and painstaking experience with Arabic, Persian, and particularly Ottoman Turkish sources. Seldom degenerating to catalogue-style citation of sources, *The Muslim Discovery of Europe* is both impressive and useful as a contribution to knowledge in a hitherto little explored area. Read in tandem with Norman Daniel's *Islam and the West*, though not Edward Said's *Orientalism*, which treats a later period, it will provide a comprehensive overview of the picture each society held of the other.

Unfortunately, the tenor of Lewis's exposition is derisive and condescending toward Muslims to such an extent that the book's analytical value is seriously undermined. One may start with the title. Why suggest a comparison between an explosion of knowledge and curiosity in Europe and a tepid lack of interest in the lands of Islam if not to show the latter to be deficient? A comparison of language skills and firsthand experience between Western Europe and the Islamic world at the present day would show an enormous imbalance in the opposite direction. The author maintains that the world distribution of power is a sufficient explanation of the fact that now more Arabs know English than Englishmen know Arabic; but different levels of economic, political, military, and cultural achievement in times past, while mentioned as partial explanations of the slight regard earlier Muslims had for Western Europe, in no way damp the repeated intimation that the Muslims *should* have been more interested in things Western and would have been had it not been for their stultifying religion and social customs (pp. 299-303).

In arguing thus, Lewis is helped by the fact that he is only examining one aspect of a many-sided phenomenon. He speaks, for example, in a chapter on the Muslim view of the world, of the division of

the world in Islamic law into a House of Islam and a House of War. "Most Muslim jurists held that it was impossible for a Muslim to live under a non-Muslim government" (p. 66). This more than the hostility of Europeans toward Muslims living in their midst is adduced to explain why so few Muslims voluntarily sojourned in Europe. Yet a broader perspective would have revealed that large Muslim communities existed for centuries under non-Muslim rule in China, Africa, and Indonesia. Indeed, in many areas such communities ultimately achieved the conversion of the indigenous population, contrary to Lewis's implication that the Islamic religion propagates primarily by warfare (p. 301). Such a perspective would also have provided a desirable comparison between Muslim interest in Western Europe and in other foreign lands.

The contrast between European bigotry and religiously dictated Islamic tolerance of Christians and Jews is well brought out, but the significant consequence cited is not that Muslims had good reason to avoid Europe but that they had indigenous minorities upon whom they could rely, when necessary, for knowledge of European languages. For Lewis, a Muslim who evidences knowledge of a Western language is not really to be counted as a Muslim if he is himself a convert to Islam or even the son or grandson of a convert. "By the second quarter of the nineteenth century, the number of [Muslims] able to read a European language was still remarkably small, and many of them were converts or sons or grandsons of converts from Christianity or Judaism to Islam" (p. 303). The implication is that Christian and Jewish mental vigor can persist genetically for some time against Muslim torpor.

Language knowledge, and particularly knowledge of foreign literatures, looms here as the highest pinnacle of altruistic intellectual attainment. "It was not until Renaissance and post-Renaissance Europe that human society for the first time developed the sophistication, the detachment and, above all, the curiosity to study and appreciate the cultures of alien and even hostile societies" (p. 75). This orientalist ideal, which Lewis himself personifies, is one he finds sorely lacking in Muslims. The fact that "educated Turks knew Arabic, Persian, and Turkish" (p. 72) counts for little in his estimation compared with their lack of interest in "foreign"—specifically Western European—languages. There is no mention that Arabic, Persian, and Turkish, belonging to three entirely different language families, pose greater intellectual challenges in their acquisition than are encountered in learning *all* Western European languages. The Muslims are simply written off as uninterested and unskilled in foreign language.

With respect to literature, the structure of the book gives rise to a curious argument. Separate

chapters are devoted to the scant data indicating Muslim knowledge of Western religion, economics, government and justice, science and technology, cultural life, and social and personal behavior. In treating cultural life, Lewis duly notes a marked Muslim interest in European art from the sixteenth century onward as well as occasional signs of appreciation of European music, which finally made a definite impact early in the nineteenth century. Only later does European literature come into the Muslim ken. In explanation Lewis avers that "for the visual and musical arts, all that was needed was to see and to hear and to achieve the measure of understanding necessary to follow the one or the other. Difficult as this might be, it was less so than the problem of mastering a foreign language or even of acquiring the desire to do so" (p. 275). Any professional artist or musician would surely dispute this proposition, but in context it poses a more profound problem. Since Muslim art and especially music have, to this day, received far less attention from Western scholars than has Muslim literature, what has caused this apparent Western deficiency in simple seeing and hearing, not to mention in curiosity? The structure of Lewis's argument would suggest that it must be Christian religious doctrine.

It seems difficult for Lewis to find something to praise in the activities of Muslims, and given that he has chosen to write about things that the Muslims virtually ignored, it is seldom necessary. But by attributing all Muslim inadequacies to their religion and entirely ignoring the realms of social and comparative history in seeking explanations, he renders the past century of true Muslim discovery of Europe difficult to understand. Today one can travel from end to end of Saudi Arabia, a rich and rigidly Muslim country that has never been ruled by a Western power, and speak nothing but English. Is our understanding of this sort of phenomenon really advanced by statements like "dealing with infidel foreigners was a dirty and dangerous business and best left to other infidels" (p. 105)?

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FRED MCGRAW DONNER. *The Early Islamic Conquests*. (Princeton Studies on the Near East.) Princeton: Princeton University Press. 1981. Pp. xviii, 489. \$35.00.

This book deals with two questions relating to the expansion of the Arabs in the seventh century A.D., that is, the origin and nature of the conquests (why and how did the Arabs attempt them?) and the course of these conquests in Syria and Iraq. It does not discuss the conquests of Egypt, upper Mesopotamia, Iran, North Africa, or Spain, and it has

practically nothing to say (certainly nothing new) on Arab strategy and tactics, the reaction of the Byzantines and Persians to the unexpected threat, the attitude of the local populations to the invaders, or other questions of relevance for the extraordinary success of the Arab venture. It also lacks a systematic discussion of the sources, a question to which the author hopes to return elsewhere.

The central thesis of the book is that contrary to the view of early twentieth-century historians, the conquests cannot be explained in terms of materialist factors such as dessication, overpopulation, or hunger. They were a highly coordinated phenomenon, not a chaotic *Völkerwanderung*; they were aimed at the establishment of political control, not at the seizure of land; the activities of the armies were determined more by the state than by the soldiers themselves; and the early armies were small—large-scale migration only seems to have started after the decisive victories. The conquests can thus be explained only as the direct result of Muhammad's career: Muhammad "revolutionized both the ideological bases and the political structures of Arabian society, giving rise for the first time to a state capable of organizing and executing an expansionist movement" (p. 8); the conquests represent an unparalleled "organizational breakthrough" (p. 269). This interpretation is perhaps not so new as the author would have it (few scholars have been so perverse as to deny that the Muslim conquests have something to do with the rise of Islam), and the degree of control attributed to the state also appears exaggerated at times. But the argument is presented with new evidence and emphases, and it is convincingly set out as far as it goes.

Whereas the organizational revolution is well documented, however, the book is surprisingly poor on the ideology that set this revolution going. There is a lengthy summary of Muhammad's life and work and a brief discussion of why the Arabs wanted to conquer at all, but no systematic examination of Muhammad's preaching on the subject of conquest or of other evidence (Muslim or non-Muslim) of what the Arabs took themselves to be doing when they set out to subdue the world; the very notion of holy war is discussed only in a footnote. The suggestion that the elite adopted an expansionist policy in pursuit of commercial interests (p. 270) is scarcely more persuasive than the discarded theory to the effect that the masses adopted this policy in search of *Lebensraum*. And on the crucial question of interaction between ideas and environment there is nothing to learn.

The rest of the book is considerably less satisfactory. As regards the author's desire to reconstruct the course of the conquests of Syria and Iraq, it cannot be said that much of an attempt is made to realize it. We are told in a priori terms that the

sources do not permit a reconstruction of the actual sequence of events; the reconstructions given in the Muslim sources are spurious, the true context of the information having been forgotten, and the non-Muslim sources give us only sketchy information, which moreover may have been derived from the Islamic tradition itself (p. 142). We are thus apparently to take it that the reconstructions of the Muslim sources are less reliable in connection with the conquests than they are in connection with Muhammad's campaigns or the unification of Arabia (where the author evinces no source-critical doubts); one might have expected it to be the other way round. And if the non-Muslim sources are too unfamiliar with Muslim ideas to be used for the reconstruction of the rise of Islam (a view to which the author clearly subscribes), they can hardly at the same time be too familiar with the Islamic tradition to be used for the reconstruction of the process whereby non-Muslim territory was lost. In fact, of course, there is no question of such sources as Pseudo-Sebeos or the Passion of the Martyrs of Gaza (the latter central to the question of the dates of the fall of Gaza and Jerusalem) retailing information derived from the Islamic tradition, yet neither source is even mentioned in this book. And the suggestion (p. 144) that the Syriac fragment deemed by Nöldeke to be contemporary with the conquest of Syria might derive its dates from the chronology of al-Wāqidī, who died almost two centuries later, is absurd, among other things because the crucial date provided by this fragment is that of the battle of Gābhithā (Jābiya), a battle that is not mentioned under this name in the Islamic tradition at all and that was only shown to be identical with that known in the Islamic tradition as the battle of Yarmūk by modern scholars. It is thus hard to avoid the impression that the author's sudden onset of source-critical doubts is an excuse for his failure to undertake the (admittedly formidable) task of re-examining the chronologies proposed by de Goeje and Wellhausen. The reproduction of the various versions given in the Muslim (and occasional non-Muslim) sources together with a general summary certainly does not amount to a reconstruction of the course of events "as definitively as the sources will allow" (p. ix). It is, however, typical of a general tendency to gloss over problems rather than to probe. (Why, for example, is there no discussion, in connection with Khālid b. al-Walid's desert journey, of Ibn Ḥabīb's unorthodox claim that this journey went from the Yamāma rather than Iraq to Syria [see *Muḥabbar*, p. 190]?) This part of the book is of interest only for the attention paid to the tribal composition of the armies involved; in general the author is at his best when he discusses the history of tribal groups.

The remainder is background information of a

conventional kind reproduced at inordinate length and subject to occasional lapses. *Hijra* does not literally mean "settling" (p. 79 and elsewhere, with serious implications for the argument). The use of modern literature on Arabia is in principle commendable, but in practice uncritical and frequently quite irresponsible (the innocent reader is led to believe that even the term *manṣīb* in the sense familiar from modern South Arabia is pre-Islamic). All in all the book is not without value, but it is too unoriginal in outlook, too long, and too ambitiously set out for what it has to offer.

PATRICIA CRONE
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GEORGE MAKDISI. *The Rise of Colleges: Institutions of Learning in Islam and the West*. Edinburgh: Edinburgh University Press; distributed by Columbia University Press, New York. 1981. Pp. xiv, 377. \$38.50.

This is a very meritorious book, distinguished by its wide scope, meticulous documentation, and thoughtful, almost enthusiastic (not necessarily always convincing) argumentation. It is the ripe fruit of many years of labor. In 1961 George Makdisi published a massive study on Muslim institutions of learning in eleventh-century Baghdad, preceded and followed by other papers in this field. In October 1976 he convened, with the assistance of others, an international colloquium on medieval education in Islam and the West. He put on the map of Islamic studies the name of Ibn 'Aqil (d. 1119), a member of the Hanbali School of Islamic law. In the first edition of the *Encyclopaedia of Islam*, Ibn 'Aqil did not even get an entry (read his interesting biography, by Makdisi, of course, in the second). But his work, especially the *Funūn*, an autobiographical notebook on disputations and other learned (and not so learned) events, gave Makdisi the opportunity for in-depth research on the whole gamut of Islamic law, theology, and education.

The Rise of Colleges opens with a detailed description of the origin of Islamic law and its teaching. The reader might wonder whether there existed any connection between this new creation and the Christian, Jewish, and pagan schools of religious teaching, existing at that crucial time, especially in the country now known as Iraq and then the center of the nascent Islamic civilization. Reading Makdisi's book, one is reminded of parallel phenomena, for instance, in the Jewish system of studies originating in late antiquity and carried over into Islamic times. It appears to me, however, that it was a wise decision to disregard this aspect of the rise of the colleges altogether. One cannot undertake everything at one time. I believe even that Makdisi's detailed exposi-

tion will contribute to a better understanding of the schools preceding Islam.

The economic underpinning of the stupendous growth of Islamic higher education, the *waqf*, or system of pious foundations, is studied next. Pious foundations and their impact on learning are to be observed also in medieval Eastern and Western Christianity. But for special reasons, largely due to the necessity and endeavor to counteract the effects of a rigid Koranic law of inheritance, this Islamic institution of pious foundations reached dimensions unprecedented elsewhere.

The reader will be particularly grateful for the lucid account, illustrated by many quotations in both Arabic and English, of the organization and methodology of learning and the composition of the scholastic community. Sometimes the "synchronistic" use of a terminology spread over centuries might raise eyebrows. Here again the author's method is to be commended. First, an inventory of the terms used and a study of their meanings must be made. Thus *mushtaghil* (see index, p. 370, occurring often) has various facets of connotation as the attentive reader of this book will notice. In a letter to Moses Maimonides (d. 1204), a man using this word wants to study medicine with him in private. "Diachronic" treatment, the historic development of usage, however, is a study by itself. Makdisi notes repeatedly that there is room for further research. (See Gary Leiser [a student of Makdisi], "Hanbalism in Egypt," *Studia Islamica* 54 [1981]: 155-182).

The chapter on Islam and the Christian West, occupying approximately one-sixth of the book and going as far west as Dartmouth College (pp. 230-35), is most stimulating and a proof of the author's thoroughness. A medievalist whom I asked to read it and to comment on it for me took exception to some conclusions, but regarded the whole as fascinating and instructive.

The book is beautifully produced. In view of the countless Arabic quotations there is no wonder that some letters, dots, bars, and raised commas are misplaced. Such slips will not irritate readers who are familiar with Arabic, nor be observed by those who are not. "Each" on page 25, 1.4, should be replaced by "Eche," an author mentioned on the preceding page. Arabic *damana* on page 16 for *damina* is perhaps a modern way of pronouncing the word.

Finally, the busy reader is advised not to skip (as I did first) over the biography of 'Abd al-Latif al-Baghdādī (d. 1231), a physician by profession, but intrinsically a master of all Islamic sciences (pp. 84-88). I recommend in particular his remarkable guide for the seeker of knowledge (pp. 88-91).

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CARL F. PETRY. *The Civilian Elite of Cairo in the Later Middle Ages*. (Princeton Studies on the Near East.) Princeton: Princeton University Press. 1981. Pp. xxiv, 475. \$34.50.

This pioneering study of the *ulema* (religious scholars of Islam) of late medieval Cairo employs a quantitative analysis of 4,631 individual biographies, which produced 45,000 separate computer cards, extracted from the two fifteenth-century biographical dictionaries of al-Sakhawi and Ibn Taghri-Birdi. Emerging from the vast amount of data and analysis, interspersed with a total of 27 figures, 11 tables, and 58 pages of appendixes, is the clearest picture we have of the background and career orientations of this religious class.

Carl F. Petry has been able to identify several differentiated and specialized groups within the body of *ulema*, such as bureaucrats (drawn largely from the major cities of the Mamluk empire), jurist-scholars (recruited from an even wider range of Egyptian and international scholars), and religious functionaries (drawn mostly from Cairo and the Delta). He argues persuasively that, while there was frequent crossover, these were basically separate career lines that drew their personnel from distinct social and geographic backgrounds. The specialized nature of the training and functions of each group made it unlikely that members could easily cross professional lines. He finally relates how each of these distinct groups developed unique relations with the Mamluk military caste that dominated this society.

The author has made judicious and imaginative use of his sources. He has noted the peculiar nature of the biographical data they contain, has been careful to point out their insufficiency for some of his purposes, and remarks those occasions when his data produced illogical results. He also presents a great deal of welcome information on the Cairene institutions to which the *ulema* were attached but is apparently unable to name those institutions in the delta to which he alludes in chapter 2.

This book will take a prominent place among studies on the *ulema*, but it has some conceptual problems. Petry has coined the phrase "civilian elite" to include the bureaucrats among the *ulema* but does not clearly define the distinguishing characteristics of this "elite." He deals with a familiar segment of Islamic society whose group identification is defined by training and function, not by social position, economic or political influence. This group is embedded vertically in society, including at the top chief justices and muftis and near the bottom repetitors in schools and blind Qur'an reciters. It is difficult to think of these lower ranks as part of an urban elite in the normal sense of the term.

The inclusion of the bureaucrats within the *ulema*

cadre tilts the statistics and influences Petry's conclusions, but we must ask if secretaries and archivists, simply because they had early religious training, were really considered to be *ulema*. Petry notes that the bureaucrats were recruited from different social and geographic regions (Copts and Syrians) than the other religious groups of Cairo.

This study also excludes the merchants, many of whom had close access to the Mamluks, played an important urban, civilian role in Islamic society, and ought to be considered part of an urban elite. Petry admits the lack of information on merchants in his sources and produces only a few examples of merchants who entered *ulema* ranks. But he remains silent on the *ulema* who engaged in business and commerce, and thereby denies us an analysis of an important aspect of the total activities of the *ulema*.

These reservations aside, this remains a stimulating analysis of a vast amount of traditional data. Petry's systematic analysis provides new insights into the nature of the group of *ulema*, their relations with the Mamluks, and the religious institutions of Mamluk Cairo. This book is required reading for anyone interested in the Muslim religious class or Islamic societies.

DANIEL CRECELIUS
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DANIEL CRECELIUS. *The Roots of Modern Egypt: A Study of the Regimes of 'Ali Bey al-Kabir and Muhammad Bey Abu al-Dhahab, 1760-1775*. (Studies in Middle Eastern History, number 6.) Chicago: Bibliotheca Islamica. 1981. Pp. xii, 201.

Daniel Crecelius has produced a work of revisionary historiography on late Ottoman Egypt. He argues that despite the presence of the Ottoman Qadi and other functionaries in eighteenth-century Cairo, and despite a certain amount of political instability in the period, which has previously attracted attention, a state began to emerge within the Mamluk system. "Ali Bey and Muhammad Bey in the span of a decade had broken the hold of the Ottoman Empire over the military, financial and bureaucratic institutions of Egypt." The main parts of the study detail the new foreign policy, including the contact with Europe. Crecelius thus argues, as I did in my book, the *Islamic Roots*, against the coming of the West in 1798 as the major rupture. Crecelius based his study on documents in the Ministry of Awqaf in Cairo. He had previously worked on the *waqfiya* of one of the principals of this study. He also used the *shari'a* court records, as well as eighteenth-century material from the French archives consular and commercial correspondence, as well as PRO and India Office material from Great Britain. What is

noticeable is the growth in the quantity of material brought together for the period studied and the great progress toward a political history that this represents over the earlier works of Holt, Ayalon Shaw, and Ramadan.

The arguments that the author makes have an impact not only on historians who work on political history but also for those who do social and economic history. Crecelius has arrived at the idea of a mid-eighteenth-century watershed with little or no involvement in the debates over capitalism and socioeconomic change; it is straight political and institutional history. Political historians may find the work hard to accept, for, as Rifaat Ali Abou el-Haj has argued, many have a stake in an "anti-Ottoman" paradigm. If Crecelius is right, henceforth it will not be so easy to speak of Turks as the cause of decline and to speak of the coming of Egyptians as the basis of progress in the nineteenth century. As Abou el-Haj argued in his article, for more than a generation, Western scholars interested in nation-state building and Egyptian nationalist scholars have used the Ottoman period as a negative image and ultimate culprit of Egypt's contemporary ills. While this is not to suggest that the Muhammad Ali period did not bring change, rather the whole historiographic tradition has been conceived to stress Muhammad Ali's Phoenix-like character. Thus, for example, 'Abd al-Rahim stressed how the peasants were oppressed in the eighteenth century, while his colleagues working on the nineteenth invariably study their themes with optimism about the future. Crecelius's work raises some fundamental questions; it encourages further research as well.

PETER GRAN
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JACQUELINE S. ISMAEL. *Kuwait: Social Change in Historical Perspective*. (Contemporary Issues in the Middle East.) Syracuse: Syracuse University Press. 1982. Pp. xii, 202. \$22.00.

If one had supposed that the oil- and capital-rich states of Arabia would be immune to the application of dependency theory by virtue of their surplus wealth, Jacqueline S. Ismael's tautly written new monograph on Kuwait is cause for reconsideration. "Is dependency a result of capital shortage, or is it a result of class dialectics that obviate autonomous capitalist development" she asks (p. 157); and her answer, based on a careful comparative analysis of Kuwait's pre-oil and post-oil society, is emphatically the latter.

To be sure, the mode of expression and the vocabulary are sometimes difficult for Anglo-Saxon empiricists to understand (for example, does her frequent use of "specify" mean "cause" or "indicate"?), and the effort to force the Kuwaiti experi-

ence into the "objective" worldwide historical process of "the development of underdevelopment" will seem to some Procrustean. Nevertheless, Ismael's closely reasoned exposition is a serious and, to my mind, successful interpretation of Kuwait's political economy that ought to stimulate debate, not to say controversy, among Kuwaiti and other Arab scholars as well as Westerners.

Ismael introduces her study with a succinct statement of the dependency theory approach she intends to follow, drawing particularly upon the work of Amin, Cardoso, and Gunder Frank; and her conclusion not only draws together her findings on Kuwait but also offers some pertinent comments on the relevance of the Kuwait case for dependency theory in general. The study itself is sensibly divided into two parts—pre-oil and post-oil Kuwait—and uses mainly Arabic histories of Kuwait, British diplomatic documents and memoirs, and Kuwait government statistical publications. If there is a certain distance from the substance and flavor of Kuwaiti politics—Ismael's main actors are classes, interests, and ideologies, not people—perhaps it is compensated for by the austere clarity obtained from applying a coherent paradigm from afar.

She persuasively documents the transformation in the pre-oil period of Kuwait from a relatively autonomous tribal-commercial society based mainly on pearling to an economic dependence on Britain's expanding imperial interests. Paralleling the economic shift, she notes, "was the transformation of the basis of Sabah authority from a tribally mediated form of community consensus to an externally mediated basis of power. Backed by the British, Mubarak transformed himself from a community leader into an autocratic ruler" (p. 55). Ismael implies, but not entirely convincingly, that Kuwait could have developed an alternative model of indigenous capitalist production and regional commerce but instead gave way to British economic and political pressures. The collapse of the pearl industry, the discovery of oil, and the decline of formal colonialism transformed Kuwait's economic and political environment, yet, according to Ismael, did so "in the framework of the continuity of dependent relations of production" (p. 81). Oil did not "liberate" Kuwait but only strengthened the ruling family and committed the state to a pattern of development that actually increased its dependence on the outside for technology, skilled manpower, and consumer goods. Her chapter on "The Transformation of Kuwait" draws astutely on a variety of Kuwaiti government statistics to demonstrate increasing income inequality, skewed sectoral development, and demographic imbalances. Especially trenchant are analyses of the problem of non-Kuwaiti labor, development planning as a device for social control, and the regime's effort to buy legitimacy in the pan-Arab sphere in order to mitigate a growing "contradiction"

tion" between a native "aristocracy" and a growing expatriate labor force. Her conclusion is that capital surplus is in itself no guarantee against dependence and underdevelopment; as "an integral unit of world capitalism" Kuwait's capitalist class is part of a world class, not a national class, and as such is unlikely to respond to national and popular needs. There are, of course, other ways of looking at Kuwait—many observers see it as a more equitable, independent, and legitimate system than does Ismael—but her critique cannot be ignored. Her book is a provocative and valuable contribution to Arab and Third World studies.

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AFRICA

CRAWFORD YOUNG. *Ideology and Development in Africa*. (Council on Foreign Relations Books.) New Haven: Yale University Press. 1982. Pp. xvii, 376. \$29.95.

Crawford Young has produced an interesting and informative introduction to the relationship (or lack thereof) between ideology and development in Africa. Dividing most of Africa's regimes into three categories—Afro-Marxism, populist socialism, and African capitalism—Young traces the recent history of the better-known examples of each. Among others, Congo, Brazzaville, Benin, Madagascar, and Ethiopia represent Afro-Marxism, while Tanzania, Algeria, Ghana, and Guinea evidence the populist socialism category, and Kenya, Nigeria, and the Ivory Coast are presented as African capitalist states.

After a discussion of these types, Young moves on to examine the relationship between African ideological preference and the major powers and appraises the relationship between ideology and performance. All in all, it is a major undertaking, much of which succeeds. Generally it is a balanced effort as well, although the author seems to deal relatively harshly with those regimes in the category of African capitalism and occasionally lets rhetoric get the better of him (as when he refers to the "maelstrom of bondage").

Students of American foreign policy as well as those interested in African politics will find this an informative volume, although one whose data are aging rapidly—the statistics are mostly from 1976 or earlier.

CHRISTIAN P. POTHOLM
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R. D. PEARCE. *The Turning Point in Africa: British Colonial Policy, 1938–48*. London: Frank Cass; dis-

tributed by Biblio Distribution Center, Totowa, N.J. 1982. Pp. 223. \$29.50.

This slight book is only one of a number of books published recently that focuses upon British imperialism in Africa. Hopefully this healthy trend will continue since much remains to be done to amplify this complex subject. R. D. Pearce's work is more welcome than most since he examines the changing attitudes of the central government during a most crucial decade. His explications confirm to a large degree what some have long known: that pressure from African leaders was secondary in the decision-making process that led to independence for British African territories. The main causative factor was the change in attitude of the British government during and immediately after World War II. This, combined with near disastrous postwar economic problems, led to the acceptance of rapid devolution of authority to the Western educated elites, a group hitherto held in disdain by colonial officials. The book is particularly good when it focuses on individuals who were responsible for attitudes and policies within the Colonial Office. Sir Arthur Creech-Jones, Labour Colonial Secretary, was one of the central characters formulating the new policies. Much of the study focuses on him, although Pearce does treat officials and nonofficials who before 1945 had questioned the main lines of imperial policy. He does not neglect the influence of outside critics, such as Leonard Woolf and Rita Hinden, and he devotes a portion of the work to the effects of Lord Hailey's studies on Colonial Office attitudes.

The author is less confident when he writes of the antecedents of the postwar changes. When dealing with such items as the lack of long-range planning for political and economic development and General Smuts's ideas for grouping together British territories, he is both interesting and informative. The early sections, however, are marred by unproven generalizations. He repeats the standard litany that gives Lord Lugard undue credit for "Indirect Rule," and he ignores creative administrators, such as Sir Hugh Clifford. His treatment of the attitudes of Sir Donald Cameron and interventionist policies is questionable, as is his choice of Kenya as an example of the export of the trustee system. The opposite is true. Until the 1930s, the Colonial Office tried desperately to foist on Tanganyika the system operative in Kenya. Even more debatable are his statements about the common European views of the Africans. One can name many responsible colonial officials who did not share the view that Africans were genetically inferior. Nor does one find the feeling of boredom and the lack of support for the empire in the interwar years that the author uses as a baseline for the changes after 1938. These criticisms, although modifying the value of the analysis of the earlier period, are not enough to detract

significantly from the total worth of the book. Those sections relating to events after 1944 alone make this book a valuable addition to the literature of imperialism.

HARRY A. GAILEY
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ELIKIA M'BOKOLO. *Noirs et Blancs en Afrique en Afrique Équatoriale: Les sociétés côtières et la pénétration française (vers 1820–1874)*. (Civilisations et Sociétés, number 69.) New York: Mouton, for the École des Hautes Études en Sciences Sociales, Paris; distributed by Walter de Gruyter, New York. 1981. Pp. 302.

Elikia M'Bokolo's detailed study of a small and generally disregarded portion of European colonial activity in the nineteenth century proves what scholars like Henri Brunschwig have long asserted: French trade often did not follow the flag. The flag waved listlessly over the pieces of Africa which would later be gathered together in the *Afrique Équatoriale Française* (AEF). However, the administrative center in Senegal was not interested in that region in the middle of the nineteenth century, and large firms, like Régis Frères of Marseilles, thought the investment of time and money there was an unprofitable undertaking. As late as 1867 the Minister of the Navy wrote: "Our commerce has only insignificant proportions in Gabon" (p. 200).

The reasons for this desultory activity are those which make M'Bokolo's study significant. Reviewing the commercial rivalry in the area (if "rivalry" is not considered too strong a term), examining the relationship between the naval officers and the few locally based merchants, and carefully analyzing the political interaction between European and African, M'Bokolo reinforces the generalizations that have been made of French activities in Africa during the nineteenth century. The study is therefore corroborative, not innovative, but nonetheless valuable for that.

There are two essential historical activities at play: Anglo-French rivalry, with its Gallic quality of Anglophobia; and the maneuvering between the French and the Africans, in which the major segments of the Mpongwe, gathered around the shrewd Chief Glass, sought to maintain their pre-occupation advantages. The two activities were interrelated, because the Mpongwe sought to pursue a *Realpolitik* by using the European rivalry to their advantage. M'Bokolo clearly defines the three periods through which Mpongwe resistance went: first, observation; second, petitions to French authorities; third, open resistance of a militant sort that resulted in the defeat of Glass. The resistance intensified whenever British and American ships arrived on the coast.

Here, as in the Niger Delta, so well described by Kenneth Onwuka Dike in his study, *Trade and Politics in the Niger Delta, 1833–1885*, the shifting pattern of European trade was to the disadvantage of the Mpongwe middlemen who soon lost their basic economic function. However, there seemed to be little advantage to the French. Their trade never progressed to the point of being profitable. The territorial expansion undertaken by various commanders in the Gabon was designed to improve the condition of French trade, which remained uncompetitive with that of the British, and, at the same time, "to prove to those who doubted it that Gabon . . . could become a useful colony" (p. 177). It did not at the time, nevertheless. In effect, what occurred was that "France paid the political expenses for the commercial profits realized by the merchants of other nations" (p. 197).

M'Bokolo is a meticulous historian who has made the most of scant research materials. His study proves once again the value of detailed accounts of the complex interaction of politics and commerce that was such a significant part of African history in the years before the "scramble."

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GERD SPITTLER. *Verwaltung in einem afrikanischen Bauernstaat: Das koloniale Französisch-Westafrika, 1919–1939*. (Beiträge zur Kolonial- und Überseegeschichte, number 21.) Wiesbaden: Franz Steiner. 1981. Pp. 208. DM 38.

Gerd Spittler, a sociologist at Freiburg University, presents in this short, readable book a fresh and thoroughly enlightening perspective on the functioning of a bureaucratic system in a traditional agrarian society. While illustrated here by the specific experience of French West Africa during the interwar decades, the author's thesis is germane to contemporary issues of administration and development in Third World countries.

The essence of Spittler's argument is that bureaucratic administration, a product of highly integrated industrial societies, cannot function effectively in the very different setting of a loosely structured peasant society. In West Africa, especially, the vast heterogeneity of languages, cultures, agricultural systems; the high degree of household self-sufficiency; and low level of literacy tended to subvert efforts to govern according to the abstract norms and rules established in distant administrative centers.

West African peasants responded to the demands and exactions of an incomprehensible system with strategies of passive resistance and avoidance. The administration lacked sufficient resources and, above all, dependable information on even the most basic matters. (The author shows clearly, for exam-

ple, that the population figures of French colonial "censuses," despite a specious precision, must be regarded as the roughest of approximations.) Bureaucratic rule as a result tended to degenerate into forms of governance more appropriate to a peasant society; arbitrariness (which does not depend on written records and abstract information); despotism (in which force becomes the most common sanction due to the *weakness* of a government that disposes to no others that would be effective against economically self-sufficient households and small groups); intermediary rule, in which the bureaucracy contents itself with reaching a level of middlemen (the canton and village chiefs), who deal with the mass of the population in an arbitrary manner and through systems of clientage, while monopolizing and manipulating essential information and thus establishing a strong, albeit entirely informal, power position of their own.

Thus taxation, equal and reasonable in bureaucratic theory, degenerated into arbitrary tribute, a labor tax for public purposes into an uncontrolled system of forced labor, military recruitment into annual manhunts. The bureaucracy became encapsulated and increasingly preoccupied with its own procedures. Reports and statistics covered profound ignorance with wholesale inventions, on the basis of which colonial ministers and governor generals prepared elaborate plans. Their inevitable failure was then ascribed to the congenital shortcomings of the stereotypical "native." Spittler's account of one of the earliest massive agricultural development schemes in Africa, the *Office du Niger*, would be profitable reading for today's development planners.

Based on colonial archives, official publications, and a large literature, including, especially, autobiographical and other works by French colonial officials, this book makes an important contribution to our understanding of the colonial situation.

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LEONARD THOMPSON and ANDREW PRIOR. *South African Politics*. New Haven: Yale University Press. 1982. Pp. xiii, 255. Cloth \$25.00, paper \$5.95.

This insightful, nonpolemical, and extremely well-written book provides the most lucid, concise, and up-to-date introduction to the South African political system and how it operates today. It is a successor to Leonard Thompson's *Politics in the Republic of South Africa*, published in 1966.

The two works are nearly identical in organization and subject matter. Chapter headings remain unchanged, and some passages come directly from the earlier version. But this does not mar the book's quality, message, or impact. Indeed, the two works

should be read in seriatim, for the 1982 volume is a dramatic continuation of the tragic unfolding of events in what the authors describe as "the one glaring example of a national political system devoted to the perpetuation of white domination in a period of history when racial domination is generally recognized to be morally indefensible" (p. 4).

After reading both books, one is struck by what little substantive change has occurred in the patterns of racial domination over the last sixteen years. The cast of characters is different, but the stage settings and scenarios are remarkably similar. Government policies and tactics have been revised, but the central premise of white supremacy is unchanged.

In 1966, Thompson prophetically concluded that "For the foreseeable future, the political system of South Africa seems likely to remain substantially as portrayed in this book—an anachronistic survivor of the colonial era, a continuous source of embarrassment to the West, and one of the most blatant flaws in the body of mankind" (p. 221). Sixteen years later, Thompson and coauthor Andrew Prior predict a "continuation of the spiral of black resistance and white repression, probably with increased violence . . . for an indefinite period [leading to an] . . . eventual radical transformation of South African society" (p. 245).

In the book's introduction, the authors dispassionately present the case for and against the government and its political objectives. Chapter 2, "The Context of the Political System," emphasizes that South Africa is racially and economically a highly stratified society where demographic trends are extremely important. White and mixed-race population growth rates are fast declining in the face of explosive growth among Africans. All population groups are rapidly urbanizing, and the authors predict that the society of the future is to be found in the towns. It is there that the political struggle will be determined.

Chapter 3, "The Framework of Political Life," focuses on the franchise issue and describes the systematic efforts since 1910, through legislative action, to disenfranchise the blacks, "coloreds," and Indians. We are reminded that the government is elected by a racial oligarchy comprising only one-sixth of the population. Real authority, they argue, lies with the executive branch; and parliament has little power but to give legislative effect to the will of the prime minister and cabinet. South Africa is a unitary state, and central government reaches into every province, into every unit of local government, and into the everyday lives of families and individuals in almost totalitarian fashion. The chapter also briefly describes the Supreme Court, the "Homelands" policy, black local government, and the country's political parties. They point out that since the mid-1960s the most serious threat to the ruling National Party (NP) has come from the right within

its own (Afrikaner) ethnic base. With keen insight, Thompson and Prior note that ironically "if the NP were to attempt to introduce changes sufficiently radical to ensure a peaceful settlement of conflict within South Africa, it would probably do so at the cost of its own position" (p. 106).

The fourth chapter, "How the System Works," identifies the major socializing agents responsible for shaping attitudes and shows how they operate and their effect on government policies. Considerable attention is given to the educational system, which, now under tight government control, upholds racial inequality. The authors observe that "Whereas in most countries the educational system is a means of incorporating all children into a common nationalism, the South Africa educational system aims to develop and perpetuate ethnocentric differences" (p. 112). This chapter also surveys the backgrounds of the country's political leadership and the role of communications networks, churches, and social organizations in articulating interests. Recent reforms in apartheid, particularly on the labor front, are treated only summarily and are dismissed as cosmetic or as new, more subtle, methods to extend government control over people and organizations. The chapter concludes with a closer examination of the political parties, noting that "South Africa has a two-party system tending toward a one-party system within a racial oligarchy or pigmentocracy" (p. 167).

Logically, chapter 5 follows with a look at "The Internal Opposition" before and after 1948. Subtopics include white opponents of apartheid, and black revolutionary leaders, their organizations, and tactics. The authors point to a "tremendous accumulation of legalized coercive powers in the hands of the executive government and a system of legalized tyranny not unlike that in the Soviet Union."

In the sixth and final chapter, "The External Opposition," Thompson and Prior remind us that South Africa is the only nation on earth that "still practices racism without shame and even raises it to the level of a philosophy" (p. 221). Within the context of that statement, opposition expressed by the Western powers is depicted as "long on rhetoric and short on substance." The U.S. and its allies have substantial interests to defend and therefore tacitly support the political system.

Thompson and Prior have admirably presented an exciting and thought-provoking narrative of the political process in South Africa since 1910, when the country became united under a single constitution. Lamentably, for reasons of brevity and contemporaneity, they fail to articulate the evolutionary process or to give a historical dimension to the present dilemmas. Little attention is paid to the dynamics of African authority structures prior to

white domination or to the enduring theme of African collaboration. Demographic conditions are analyzed, but not the actual population movements. The Mfecane is not mentioned, nor is the enormous impact of the immigration of European Jews. The book is packed with fascinating and relevant facts and events, but more should be said of the subtle human motivations that generated them.

Like most surveys of South Africa, the book suffers from a paucity of illustrative and statistical material. There are only two maps, fourteen tables (all post-1970), and no graphs or pictures. It also lacks a bibliography or guide to further reading. Nevertheless, *South African Politics* is a vitally important book and will be useful to college undergraduates and laypersons involved professionally with South Africa. It should make a significant contribution to the growing public awareness of South Africa's problems.

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ASIA AND THE EAST

ALBERT CHAN. *The Glory and Fall of the Ming Dynasty*. Norman: University of Oklahoma Press. 1982. Pp. xxx, 428. \$24.95.

This work by Albert Chan originated as a 1953 Harvard dissertation analyzing internal causes of the decline and fall of the Ming dynasty. It was later expanded, but it remains essentially an interpretation of what was wrong with Ming China, principally in terms of its political and socioeconomic institutions and the morality they spawned. Few Ming glories are discussed, and most of these are attributed to the founding emperor, whose achievements in stabilizing state and society, we learn, were steadily undone after him by inept rulers, corrupt officials, cruel eunuchs, greedy imperial kinsmen, rapacious gentry landlords, a useless military establishment, hypocritical and stagnating literati—and by the inflexibility of the very institutions for which the founding emperor is praised.

This now familiar argument is presented in topical chapters full of readable anecdotal material drawn principally from late Ming and early Ch'ing writings. Had the book been published in the 1950s, it might have been welcomed as an earnest effort, with some flawed understandings, to sum up the state of the field. However, it appears after a generation of high-quality research that Chan in some cases acknowledges in notes added to his introduction but has not actually digested. "The only noteworthy recent work on the revolts of the late Ming period," we read, "is the *Wan-Ming min-*

pien by Li Wen-chih" (p. xxv), a 1948 publication. In three chapters devoted to economics, the most recent Western-language work cited in annotations is the general history of China by J. B. du Halde (d. 1743). Specialists might be relieved by the consequent absence of the kinds of references to "sprouts of capitalism" and "feudal abuses" that abound in much recent writing about Ming, but they can only regret the author's failure to make use of the work of Ho Ping-ti, Mark Elvin, Ray Huang, F. W. Mote, Dwight Perkins, James B. Parsons, Lo Jung-pang, Wing-tsit Chan, George Dunne, C. R. Boxer, Donald Lach, and many others who have dealt with some of Chan's major topics—not to mention his failure to benefit from such monuments as the *Dictionary of Ming Biography* (1976), the symposium volume *From Ming to Ch'ing* (1979), the series of works on Ming thought edited by William T. deBary, or even the *Fonti Ricciane* edited by P. M. d'Elia (1942–49). The book simply cannot be considered representative of current scholarship. Anyone seriously interested in the political, socioeconomic, and "moral" problems of late Ming will do better to read Ray Huang's more informed, more ingeniously conceived, and much more perceptive *1587, A Year of No Significance* (1981).

Chan's book is enviably provided with Chinese characters inserted in the text, the notes, and the bibliography. It is regrettably marred by many erroneous romanizations, however, and nonspecialists will be confused by its recurrent use of romanizations and characters for official titles and other terms without any clarifying explanations.

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EDWARD L. DREYER. *Early Ming China: A Political History, 1355–1535*. Stanford: Stanford University Press. 1982. Pp. 315. \$30.00.

In the present work, Edward L. Dreyer has provided a reign-by-reign outline of the political and military history of the Ming regime from its pre-imperial establishment in Nanking ca. 1355 to the end of the reign of Hsuan-tsung in 1435. The author argues for this definition of the period by asserting that it "determined the final shape taken by Ming institutions."

In his longest chapter, Dreyer outlines the formation of rival Chinese polities in the Yangtze Valley, the fragmentation of the Yüan regime in the north, and the military history of the Ming unification. The point is made that the military core of the evolving Ming regime was created by the integration of originally opposed or independent armed bands into ever larger units, ultimately producing a conical

structure unified mainly by vertical ties of obligation between commanders on adjacent levels. The next chapter divides the first reign at 1380 into periods of "consolidation" and "direct rule." Before 1380, continuities with Yüan institutions are stressed, notably the creation of a hereditary military organization led by a new nobility. Also treated are the shift from a predominantly commercial and monetary tax base to an agricultural and commodity base, as well as the restriction of trade and tribute along the frontiers. The latter part of the reign began with the first of the great bureaucratic purges, assumption of direct rule, and the reinstitution of the examination system for the civil service. The period ended with the destruction of the nobility and their replacement in military commands by the princes.

The history of the Prince of Yen's overthrow of the second emperor and his program of imperial expansion are then narrated, as are the growth of civil administrative authority and the beginning of the long decline of the guard system. The transfer of the capital to Peking is characterized as "dysfunctional" in the long term but dictated by the usurper's need to rule from his old power base. (Edward Farmer's arguments in *Early Ming Government* for a functional explanation are not addressed.) The narrative ends with the Hung-hsi and Hsuan-te reigns, marked by the firm establishment of a balance among civil, military, and eunuch agencies.

In his concluding chapter, "Ming History: The Roads not Taken," the author presents his main thesis. This is roughly as follows: the subsequent institutional, social, economic, and intellectual history of the Ming was "channeled" by decisions freely taken by the emperors in response to immediate circumstances. The mode of argument is first to point to certain continuities in Ming history after 1435 and then to consider how Ming history might have been different had other choices been taken. Finally, in a kind of postscript, Elvin's concept of the "high-level equilibrium trap," presented in *The Pattern of the Chinese Past*, is introduced to explain the end of the "medieval economic revolution."

Much of this is open to objection. First, an argument for the long-term channeling of Chinese history by choices freely made by five emperors (*dei ex machina*?) demands consideration of long-term, structural limiting factors, which, indeed, is where others might look for "channels." Second, the device of "roads not taken" or "alternative scenarios" lands the author in logical difficulties. By no historical method is it possible to specify the consequences of an event that did not occur: one can only guess. Third, whatever the merits of the "equilibrium trap" hypothesis, its chronologically possible relevance to the present work depends upon a demonstration that the "medieval economic revolution" ended between 1355 and 1435. (Elvin is vague on this point.)

Dreyer's extensive reading of official historiography has yielded some reasonable generalizations on politics and some good military history, but his narrative does not bear the weight of his main thesis.

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JOHN MESKILL. *Academies in Ming China: A Historical Essay*. (Monographs of the Association for Asian Studies, number 39.) Tucson: University of Arizona Press, for the Association for Asian Studies. 1982. Pp. xiv, 203. \$12.95.

Out of several hundred academies, John Meskill chose seven of the most prestigious and well documented on which to base his study. His approach is to consider the academies only on the basis of their role as schools. Our present knowledge does not permit consideration of their general influence in the social, intellectual, and political spheres (though, in fact, the author could not avoid some reference to politics). Thus, the work might be compared to a history of one of our major universities.

Chapter 1 traces the evolution of the academies from their embryonic stage in mid-T'ang through the Sung and Yüan when they approached the full maturity attained in the Ming. Thus, he provides evidence for the connections between the Ming academies and those of Sung and Yüan, giving further proof that the Ming represented an adaptation to Chinese tradition rather than a radical break.

The following five chapters, though considering such more technical questions as the administration of the Ming academies and their architecture, are essentially devoted to discussions to demonstrate the validity of the author's central thesis that the history of Ming academies can be divided into four periods of roughly seven decades each: "relative dormancy," "revival," "dominance in the intellectual world," and "reversion to a more conventional place in the educational order." Meskill is thoroughly convincing in proving his thesis. Obviously, his favorite period is the third, dominated by Wang Yang-ming. Much has been written about Wang, yet the author is successful in adding another dimension. My only suggestion is that he divide the fourth part into two: "reversion to a more conventional place in the educational order" (first five decades) and "destruction" (final two decades). All the academies suffered sad fates indeed. The failure was particularly unfortunate for China; for, if the third period could have continued until 1644, there might not have been a Manchu conquest, and China would have been more receptive to Western ideas. Thus, the author has presented the failure of one of the most potentially promising educational movements in world history. I far prefer it to John Dewey.

My most general criticism of the work is that Meskill does not provide more consideration of some of the fundamental points he mentions. For example, though he acknowledges the influence of Buddhism (then dominant in the educational field) on the academies during the formative period, he provides little to help one answer a perplexing question: why did Buddhism lose its educational dominance?

Concerning minor details, space permits mention of only one: surely the "Professor Edward Tzuu Ch'en" (p. 182, n. 171) should read "Professor Edward Hsin-tzuu Ch'ien."

Though Meskill emphasizes the pioneer nature of his work on several occasions, it serves as a worthy foundation for future research on a particularly important and little-known topic.

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ROBERT E. BEDESKI. *State-Building in Modern China: The Kuomintang in the Prewar Period*. (China Research Monograph, number 18.) Berkeley: Center for Chinese Studies, Institute of East Asian Studies, University of California. 1981. Pp. x, 181. \$8.00.

In this book, Robert E. Bedeski attempts to identify the extent to which the Kuomintang created a modern Chinese state in the Nanking Decade (1928–37). He uses A. P. d'Entreves's conception of the state in terms of three sequential characteristics: force, or actual physical control over the country; power, the qualification and restraint of force by institutions and laws; and authority, the moral dimension of force and power that creates legitimacy. Bedeski examines military, political, economic, and social aspects of the history of the Nanking Decade in terms of those categories and concludes that the Kuomintang was not able to complete the military unification of the country—not fulfilling d'Entreves's first requirement—and therefore could not create power or authority. Nonetheless, Bedeski argues, the Kuomintang made substantial progress in creating a modern state, notably by eliminating regional militarists and by abolishing external restraints on the state such as the unequal treaties. This progress, he suggests, greatly simplified the state-building tasks of the Communists after 1949; in effect, the Kuomintang and the Communists unintentionally cooperated in establishing the modern Chinese state.

A careful appraisal of Kuomintang state-building achievements would be useful, and Bedeski's work is a step in that direction. But the work provokes argument on many points; that is, indeed, the author's intention. In this brief review, I will mention only two issues.

The sequential component of d'Entreves's (and thus Bedeski's) analysis is somewhat mechanistic. It ignores the fact that moral dimensions of a regime's institutions can influence the force it can muster. That is especially true in a revolutionary situation, and the Kuomintang and Communists offer instructive contrasts in that regard. The Communist victory was a military victory, and the Kuomintang was militarily defeated. But the Communists' moral qualities contributed to their military strength, and Kuomintang military power was undermined by the party's apparent immorality. Bedeski scorns as simplistic interpretations of the revolution in terms of Kuomintang corruption versus Communist honesty, but corruption as an element of force merits discussion.

Bedeski repeatedly asserts that Kuomintang achievements in state-building contributed to the Communists' success in creating a modern state; even political recovery after the Cultural Revolution, he claims, would have been more difficult without the earlier efforts of the Kuomintang. This may or may not be true, but Bedeski does not show that it is true. His analysis focuses on the Kuomintang during the Nanking Decade, and his ideas about later effects on the Communists are expressed as simple assertions. The relationship between the Communist state and the earlier state-building of the Kuomintang requires careful analysis in its own right, not generalizations on the basis of analysis of Kuomintang activities only.

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RAYMOND F. WYLIE. *The Emergence of Maoism: Mao Tse-tung, Ch'en Po-ta, and the Search for Chinese Theory, 1935-1945*. Stanford, Calif.: Stanford University Press. 1980. Pp. viii, 351. \$25.00.

Both in China and abroad, the few years since the death of Mao Tse-tung have seen intense efforts to come to terms with the influence of the man and his ideas on modern Chinese history. Raymond F. Wylie's book marks a considerable step forward in our understanding of the historical process through which Mao and "Maoism" achieved such enormous influence over the Chinese Communist party before and after it came to national power in 1949. This is not a biography or an abstract analysis of Mao's thought. Rather, it is a detailed and carefully argued examination of the specific historical circumstances under which Mao and his followers built up Mao's stature as a Marxist theoretician during the "Yenan period" from 1935 to 1945.

In particular, Wylie focuses on the role of Mao's political adviser, literary amanuensis, and ideologist, Ch'en Po-ta. Ch'en has remained a shadowy and enigmatic figure in the history of Chinese commu-

nism. A close associate of Mao since 1937 and a leading theoretician and propagandist for over thirty years, he is chiefly known abroad as one of the leading figures in the early stages of the Cultural Revolution before he was repudiated and rejected by Mao in 1971. Through close analysis of the published writings from the late 1930s by both Ch'en and Mao, Wylie makes a case that Ch'en was much more than a facile speech writer for Mao but also had considerable influence in the evolution of some of Mao's most important theories about class relationships in Chinese society and the cultural aspects of the Chinese revolution. Most important, Wylie sets these textual analyses in the context of the dominant political realities of the time—the anti-Japanese war, competition with the Nationalist government, and intraparty rivalries within the Chinese Communist party.

This is intellectual history, or history of a developing ideology, in its historical-political context. Wylie leans toward a practical power politics explanation for many of the specific features of the emerging body of thought called "Maoism" in the West and "the thought of Mao Tse-tung" in China. He also sees reasons of state, or politics, behind the whole phenomena of the "cult" of Mao. This is convincing to a point, but the book's biggest weakness is the lack of unofficial source materials, such as memoirs or independent reporting that might supplement the official published text. Wylie is forced to rely on random observations by casual Western visitors to Yanan or the highly suspect memoirs of the Russian Comintern observer, Vladimirov, for evidence to corroborate his inferences about the political infighting behind the ideological screen. Wylie's inferences and conclusions are plausible enough, but often the book is more retrospective Kremlinology than documented political history.

Nevertheless, on the whole, Wylie's analysis of the factors behind the rise of the cult of Mao Tse-tung is of considerable value. He shows how and why it was necessary to buttress Mao's initially suspect reputation as a Marxist theorist, to rewrite party history, and to make Mao's thought the prevailing orthodoxy for the whole movement. Wylie's speculations about an innately Chinese quality behind the Mao cult, seen in the efforts to combine theoretical with practical leadership, sage with ruler, are more questionable in view of other Communist parties' experiences in building up a mantle of ideological infallibility around the person of the leader. Still, Wylie is probably right in arguing many of the top Chinese Communist party leaders overcame their reservations about Mao's theoretical prowess because the competition with Chiang Kai-shek and the Nationalist government required their party to put up a leader with an image of being a prescient national leader. Of course, in the long run they succeeded all too well in that objective as many of them, including

Ch'en, were purged by the omnipotent leader they had helped to create.

There is a brief epilogue on Ch'en Po-ta's role in the Cultural Revolution, but the main value of this book is for the ideological and political history of the Yen-an period. It is not a verdict, final or tentative, on Mao's overall role in the Chinese Revolution. But it is an important contribution to the emergence of Maoism and the political reasons for it.

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JONATHAN UNGER. *Education under Mao: Class and Competition in Canton Schools, 1960–1980*. (Studies of the East Asian Institute.) New York: Columbia University Press. 1982. Pp. xii, 308. \$24.00.

Using Canton schools as a case in point, Jonathan Unger argues that divisions over China's modernization strategy have been most sharply focused in the area of education. For the years 1960–80, he traces the pendulum swings between emphasis on education as a development tool and emphasis on education as a redistributive-egalitarian mechanism. He recounts Chinese attempts to make their schools serve three different and sometimes contradictory roles: as places of learning, as socializing agents, and as selectors of the next generation's professional strata. He illustrates the great difficulties facing leaders in their attempt at controlled revolution.

In his research, Unger relied on Chinese newspapers and periodicals and on interviews in 1975–76 with forty-three emigrant students and teachers. Several statistical analyses of class background essential to his thesis were based on the memories of these Cantonese emigres. Although changing definitions of class and imprecise memories might raise doubts about exact percentages for such categories as "middle class non-intelligentsia" or "bad class rich peasant," the general thrust of his argument is convincing.

Two themes pervade the study: the ways that school structure influenced study habits and the conflict along class lines in student competition. Because of tracking into "key point" model schools, ordinary schools, specialist schools, and so forth, the students from culturally advantaged backgrounds gained predominance in the best high schools. When the emphasis was on expertise during the early 1960s, a school's reputation depended on the number of its graduates gaining university admission. Schools, therefore, tailored their admission policies, curricula, and teaching techniques to achieve success in the national examinations, and the students tailored their study activities to the same end. The "diploma disease" was particularly rampant in the "key point" schools.

Setting China apart from other countries suffering from the "diploma disease" was the weight given political activism in the selection process. As a consequence, cadre children could generally gain admission to the better schools with somewhat lower scores than "bad class" middle-class offspring. The latter group thus came under pressure to give proof of "redness," while many cadre youth were disadvantaged in academic competition. The more prestigious the school, the smaller the percent of "good class" worker and peasant children it enrolled.

Competition and frustration intensified as the effects of the baby boom coincided with restrictions on urban jobs opening up to the educated. According to Unger, the violence and extremism of the Red Guards during the Great Proletarian Cultural Revolution were the product both of tensions linked to class awareness and of organizational sophistication developed in building a record of political activism.

The importance attached to "redness" during the Cultural Revolution and the era of the Gang of Four certainly cured the "diploma disease." This provided, however, small comfort for Unger, who had hypothesized that students would be better able to learn, for intrinsic reasons, if education could be divorced from competition for careers and if the threat of selection examinations were removed. Rather, indiscipline, minimal concern with studies, and even political lassitude eventually characterized the students. Personal influence (that is, cadre connections) became the major determinant in mobility.

The return, in the late 1970s, to emphasis on development and the concomitant demand for expertise meant the revival of tracking, selection by examinations, and intense competition. Unger concludes pessimistically: "China finds itself back at square one. . . . Twenty years have passed; but little has been solved. China faces the same sets of dilemmas, retracing a familiar road. If anything, the problems are greater. . . . Chinese education has come full circle in two decades, only to have lost ground" (p. 217).

Unger's picture is, I think, more depressing than warranted. While China has yet to achieve a balance between education as an instrument of modernization and education as a leveling device, China is hardly back where it was in 1960. The educational infrastructure is larger and more fully developed. Implementation of birth control, industrial growth, and the fading of traditional class categories in succeeding generations may not solve all problems, but they should mitigate competition. If the great experiment of the Cultural Revolution is over, commitment to egalitarianism and the communal good remain national goals.

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HARRY HARDING. *Organizing China: The Problem of Bureaucracy, 1949–1976*. Stanford: Stanford University Press. 1981. Pp. xi, 418. \$29.50.

The subject of political bureaucracy in contemporary China brings to mind a host of characteristics that, taken together, are unique to the Chinese case: a long historical legacy of bureaucratic theories and operating principles; governmental concerns that range from social and cultural transformation to the planning and management of the national economy; the resiliency of bureaucratic structures within an often turbulent political context; and the sheer size of the Communist party and state government apparatus required to govern a society of nearly one billion people. Specific also to the People's Republic of China has been the fundamental theoretical assumption of its leaders that the issue of the role of the state and party organizations is not merely incidental to other public policy issues. As noted in the preface to *Organizing China*, Chinese leaders believe that "effective organizations . . . are a crucial prerequisite for success in any public endeavor, whether it is making a revolution or developing the economy" (p. viii). For these reasons, Harry Harding's meticulously researched and lucidly organized study of China's "problem of bureaucracy" deals with a very major issue and constitutes an important building block in a growing edifice of public policy studies of contemporary China.

Harding traces chronologically, from 1949 to the death of Mao Zedong in 1976, the way in which China's leaders have perceived and identified organizational problems, and he follows in detail the policies that they have formulated and implemented in an attempt to correct them. Harding outlines three main sets of organizational issues: "administrative" (i.e., impediments to bureaucratic effectiveness), "political" (i.e., the implications of bureaucratic organization for the distribution of power), and "philosophical" (i.e., "social dilemmas" surrounding the role of bureaucratic structures within society). Because of the strong ideological concerns of the regime, particularly while Mao was still alive, the "social dilemma" issues are more central to policy discussions in China than in most other countries and, therefore, of special interest. Harding's treatment of those issues are, in this reviewer's opinion, the strongest point of the study. In his analysis of Chinese approaches designed to deal with organizational problems, Harding formulates a fourfold typology: (1) "rationalization" (increasing the role of bureaucracy and fine-tuning its performance), used in China primarily in the mid-1950s; (2) "radicalism" (dismantling or at least minimizing bureaucratic structures), used during the Cultural Revolution; (3) "external remedialism" (mobilizing segments of society outside the bureaucracy to criticize the party

and state structures), applied during the "Hundred Flowers" period of liberalization in 1956–57, for instance; and, (4) "internal remedialism" (conducting reform campaigns within the bureaucracy).

Harding's narrative covers three decades of organizational policy shifts at a level of detail that is likely to be primarily of interest to the China specialist. His analytical framework, however, is invaluable not only to the specialist in "making sense" of the data, but also to the comparativist who may wish to relate the patterns evident in the Chinese case to other societies. Harding's concluding chapter suggests that those patterns are not simple—for example, he is compelled to expand his typology of approaches to the "social dilemma" to include five additional "mixed types." He concludes, however, that "internal remedialism has been at the center of gravity of Chinese Communist organizational policy, just as it was the center of gravity of China's imperial tradition" (p. 333).

This reviewer's only disappointment with the book is that it is not more forceful in making some predictions about the future. Beginning in 1978, the Chinese leadership clearly opted for "rationalization" in rebuilding its bureaucratic structures. And, in 1982, an extensive bureaucratic reform movement was launched, constituting what appears at the time of this writing to be further reliance on the "rationalization" approach. But what can we anticipate over the next decade or two? Has the passing of Mao signified an abandonment of "radicalism" and "internal remedialism"? Are there trends that can be expected to result from economic modernization and from the maturation of the regime? Harding has whetted our appetite for some answers.

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FRANZ MICHAEL. *Rule by Incarnation: Tibetan Buddhism and Its Role in Society and State*. (A Westview Special Study.) Boulder, Colo.: Westview Press. 1982. Pp. xii, 227. \$25.00.

This study by Franz Michael purports to describe and analyze the role of Buddhism, particularly the concept of rule by incarnation, in the former Tibetan polity. It surveys the historical development of Buddhist influence on governance in Tibet from early times to the present (pp. 1–50) and then describes the dyadic structure of ecclesiastic and secular administration on the provincial and local levels (pp. 51–110). Aspects of social order and education relevant to the Tibetan polity are explored (pp. 111–55). The final chapter deals with religious and political events leading up to, and then following, the abolition of the former Tibetan government by the Chinese regime in 1959.

Departing from Max Weber's conceptual approach (as defined in this study) that non-Western societies are "ossified by monks' bureaucracies," implying that a "church state" cannot be modernized (p. 25), this study concludes that Tibetan Buddhism and a modernized society are not incompatible and that the loss of political independence in Tibet basically was due to the lack of communication between monastic bureaucrats and military reformists in this century (pp. 166–67).

The author had a collaborator, Eugene Knez, and two Tibetan scholar-informants as assistants on this study, but he accepts the responsibility for its conceptual framework, analysis, and writing (p. xii); therefore, this review refers to the author singularly. Textual research aside, the major source of data for this study came from numerous interviews with Tibetans living in exile in India and Ladakh. Since the author does not speak Tibetan, the interviews were conducted with the aid of interpreters. According to this study, the Tibetans interviewed generally remember the former Tibetan polity as one verging on a premodern state in which almost every Tibetan had a considerable degree of social mobility.

The space allotted this review precludes discussion of the innumerable errors in dates, facts, and interpretations found in this study. The first two chapters, which survey the evolution of Buddhist governance in Tibet, are so flawed by errors and misunderstandings as to be little more than a parody of that history. The data in this study are further obscured by the rendition of Tibetan vocabulary by phonetic approximations. The only justification in a scholarly work for rendering a language phonetically is when the language concerned is that of a nonliterate people. Tibetans have been writing their language in an alphabetical script for over a thousand years, and there is no excuse for not using a proper orthographic transcription for it. Even though the author is not a Tibetologist and does not know the language, his two Tibetan assistants surely could have supplied correct orthographies for this study.

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KENNETH ALAN GROSSBERG. *Japan's Renaissance: The Politics of the Muromachi Bakufu*. (Harvard East Asian Monographs, number 99.) Cambridge: Council on East Asian Studies, Harvard University; distributed by Harvard University Press, Cambridge. 1981. Pp. xii, 207. \$15.00.

Until recently the Muromachi period (1336–1573) has been one of the least understood chapters in Japanese history. Standing at the fulcrum of change

from a polity led by the civil nobility to one dominated by a military aristocracy, it has given historians numerous interpretative problems. Chief among these has been the question of whether the period should be written off as a time of institutional breakdown—a political dark age—or recognized as a time of significant institution-building. Since the late 1940s, Japanese historians have revised the Muromachi image in a more positive light, and on the heels of these revisions new studies of the Muromachi age have begun to appear in English. Kenneth Alan Grossberg's monograph is the first such work devoted wholly to the political history of the Muromachi shogunate (*bakufu*). The monograph falls into two parts, the larger portion (chaps. 2–5) is mainly descriptive, and the remainder (chaps. 1, 6, 7) is devoted to problems of interpretation and of comparison between Japanese and European historical development. The descriptive chapters synthesize the work of the leading Japanese specialists. In them Grossberg shows good command of Japanese materials, both modern monographic and medieval printed sources.

Grossberg starts with the history of the Ashikaga shoguns. The narrative is well written, and the treatments of the three shoguns Yoshimitsu, Yoshimochi, and Yoshinori are by far the most analytically complete we have in English. One could wish that the story beyond Yoshimasa had been told in greater detail so as to explain the remarkable staying power of the *bakufu*. Also a more extended treatment of the civil and religious establishments that comprised the Kyoto court would have given a better sense of the balance of power with which the *bakufu* had to contend.

Chapter 3 deals with the economic base of the Muromachi *bakufu*. As Grossberg points out, it is now clear that the Ashikaga shoguns relied less on income from private land holdings than on services rendered by its various categories of vassals (*shugo*, *hōkōnin*), on the prerogative to collect special nation-wide taxes (*tansen*), and on levies imposed upon temples and commercial bodies. In the succeeding chapters dealing with the administrative and military organization of the *bakufu*, Grossberg lays particular stress on the use made of hereditary professional bureaucrats (*bugyōnin*) and a "shogun's army" (the *hōkōshū*).

The theoretical base on which the descriptive chapters rest is fairly modest, consisting of a number of developmental dualities—between bureaucracy and autocracy, centralization and decentralization, feudalism and urbanized capitalism. But Grossberg has written with more in mind than these loosely applied low-level generalizations. The title "Japan's Renaissance" is for him more than a catchy phrase. It is meant to be an operational construct, a political model derived from the Renaissance city-states of

Italy and the early French and English monarchies. Grossberg does not spell out in an integrated statement the parameters of his model, but the elements are presented piecemeal and can be summarized somewhat as follows: (1) the shift from a feudal to a "postfeudal autocratic monarchy," (2) the shift from an "autocratic-agrarian society" to a "capitalist-agrarian society," and (3) the emergence of an "urban-commercial" orientation. The application of such concepts is handled in two ways. First they are inserted as a series of asides in the descriptive chapters, leading to such observations as: the Ashikaga shoguns were "feudal monarchs striving toward a postfeudal autocracy" (p. 2); they started out as "rustic feudal warlords," but became "urban princes" (p. 14); they became in the end "king in everything but name" (p. 15); and the relationship between shogun and *tennō* was as between European monarchs and the pope (p. 14).

Secondly, in his concluding chapters, Grossberg makes a major effort to demonstrate the existence of a historical parallelism between Japan under Ashikaga Yoshimitsu, France under Louis XI, and England under Edward IV. Admittedly general trends toward centralization and urban-centered commercialization were common to all these societies. But before such conceptions as "feudal monarch" and "kingship" can be used legitimately across the wide cultural and linguistic separations involved here, there needs to be a more precise understanding of what such terms mean. What in the Japanese context does it mean to say that the Muromachi shoguns during their first century showed an "unmistakable drive to establish a monarchy" (p. 116)? The implication is that the Ashikaga house sought to be "king of Japan" in the European sense of monarchy rather than shogun in the fullest Japanese sense, and it leaves unresolved what to do with the *tennō*. To imply that the *tennō* served as counterpart to the European pope is no solution. The Japanese tradition of *kishu*, of hereditary charismatic status, gave to the *tennō* a position that denied the rise of a Japanese "monarch" independent of the *tennō*. This is not to deny the desirability of historical comparisons between Japan and Europe. Grossberg has suggested a number of intriguing parallels. But these must still remain as suggestions within a hypothetical frame rather than as conclusions discovered in the historical record.

JOHN W. HALL
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WILLIAM SHAW. *Legal Norms in a Confucian State*. (Institute of East Asian Studies Publications Series, Korea Research Monographs, number 5.) Berkeley: Center for Korean Studies, Institute of East Asian

Studies, University of California, Berkeley. 1981. Pp. xv, 267. \$10.00.

William Shaw's "Confucian State" is Yi dynasty Korea (1392–1910), primarily down until 1800. Since a large section of the book is devoted to a translation of cases from the *Simnirok*, a collection of hearings of about one thousand cases from 1776 and 1799, the work is most informative about the latter half of the eighteenth century.

Korea is the underdeveloped poor relation in East Asian studies, and it might be argued that Korean law has received less attention than other areas of academic inquiry such as history, religion, anthropology, and the like. Indeed, Shaw's bibliography includes not one work by a Western scholar devoted to Korean law and only a few scattered articles besides his own that relate to the topic even tangentially.

The only English-language work noted is *The Korean Political Tradition and Law* by Hahm Pyong-Choon, a Korean scholar perhaps best known as a former ROK ambassador to the U.S. In his book Hahm was intent on convincing us that Koreans disdained "abstract, uniform and unconditional rules and regulations" in favor of "the force of moral example" (Hahm, p. 44).

But Shaw's book serves to demonstrate that Yi dynasty Koreans regarded law as an "indispensable element of justice" (p. 118) that, when combined with ethical principles, could help achieve effective administration. Actually, if one accepted Hahm's view, he would be bewildered by the amount of information on Korean law and legislation Shaw has presented. With chapters devoted to judicial institutions and criminal proceedings, legal values, law-making, and legal reasoning—supplemented by a translation of 100 cases of *simni* hearings—Shaw depicts Yi dynasty criminal law in all its complexity, emphasizing that "a well-functioning legal order was considered a vital aspect of government" (p. 10). In the light of Shaw's research, Hahm's arguments seem naively idealistic.

The *simni* hearings—cases where the death penalty could be inflicted—are fascinating in themselves. They demonstrate a rather high-level concern with justice and principle; arbitrariness seems markedly absent. Leniency too was common. Shaw notes that in *simni* cases during King Chǒngjo's reign "fully one-third of the defendants were released, and even more were given sentences of less than death" (p. 126).

Likewise, the cases are a fascinating mirror of the social realities of the day, with cases ranging from treason to domestic quarrels ending in murder. Drunkenness appears to have been a contributory factor in many cases. While Koreans dispatched each other with a variety of weapons, a higher

number of homicides resulted from the victim's being kicked. Here perhaps is corroboration for the assertion of Korean martial arts masters that Koreans have a cultural proclivity for kicking, thus distinguishing Taekwondo from either Chinese kung-fu or Japanese karate.

Adapted from his Harvard dissertation, Shaw's *Legal Norms in a Confucian State* is an important book, the first Western work on the Korean legal tradition. Required reading for Korean specialists, it will also be of enormous value to China scholars, since Shaw takes great pains to discuss the areas in which Korean practices diverged from their Chinese counterparts.

G. CAMERON HURST III
University of Kansas

SUSAN LEWANDOWSKI. *Migration and Ethnicity in Urban India: Kerala Migrants in the City of Madras, 1870–1970*. New Delhi: Manohar; distributed by South Asia Books, Columbia, Mo. 1980. Pp. ix, 243. \$16.00.

This book is indicative of the growing interest in internal migration in Third World countries and in the effects of that movement on the “home” and “host” communities of the migrants. No doubt, this heightened awareness stems from the fact that the population redistribution generated by the uneven development of regions during colonial rule underlies, in part, the rise of divisive tribal, parochial, and communal movements in many new nations today.

Susan Lewandowski sets out “to trace the historical process of migration within one linguistic community [Malayalam-speaking Kerala migrants] resident in a city [Madras] located in a different cultural region, and to relate this process to the emergence of an ethnic identity” (p. v). The initial two chapters describe the conditions prevailing in Kerala and Madras that precipitated migration. The “push” factors operating in Kerala originated under *Pax Britannica* as the population grew rapidly, and many acquired Western education but found few professional opportunities at home. As the first colonial port city in India, Madras, on the other hand, became the “central-place” for the South; its “pull” over the region was exerted by the prospects it offered for jobs in higher education, government, and other professions. At the turn of the twentieth century there were already two thousand Keralites in Madras.

Chapters 3 and 4 sharpen the focus on the Kerala migrants and are richer in detail because they draw on the better data of the twentieth century. Whereas the movers of the late nineteenth century naturally included a disproportionate number of elites because of the white-collar job opportunities, in the

twentieth century, as Madras developed its commerce and industry, many migrants were of the “poorer class of Hindus, Muslims or Christians” (p. 82). By 1961 there were 58,000 Keralites in Madras. An insightful look at “The Kerala Family in an Urban Setting” (chap. 4) completes the collective portrait of migrants by sketching in information on family and kinship ties and on the different class and religious “segments” of the community.

The last two chapters treat the migrant community in relation to other groups in the city. The author's scrutiny of voluntary associations yields the familiar finding that these organizations serve as mechanisms to reinforce ethnic identity, although, as she notes, they are divided along class lines. The final chapter contrasts the relative absence of tensions between newcomers and locals in Madras with the situation in Bombay, where the nativist movement has established a strong foothold. (Here, as elsewhere, she makes instructive comparisons to the experiences of other cities and other migrant groups.) However, since 1970—the date at which her account ends—there have also been clashes in Madras. Indeed, as the conclusion states: “in the multi-ethnic society of India, ethnicity will probably remain a very important part of the political future of the nation, and may increasingly determine the allocation of economic and political power at the state level” (p. 215).

As a historical reconstruction of the elusive subject of migration, this book is exemplary; its findings, however, do not depart significantly from those of studies conducted by social scientists.

ANAND A. YANG
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DAVID HARDIMAN. *Peasant Nationalists of Gujarat: Kheda District, 1917–1934*. New York: Oxford University Press. 1981. Pp. x, 309. \$27.50.

It is well known that, in the three decades following Gandhi's return from South Africa in 1915, large numbers of peasants, in districts scattered throughout India, supported the nationalist movement by demonstrating, withholding taxes, and in some cases committing acts of violence. Gradually we are learning who these peasants were and how they were motivated and mobilized. David Hardiman has made an excellent contribution to the explication of this political process.

The backbone of the successive noncooperation and civil disobedience movements in Kheda were the middle and rich peasants of the Patidar caste, a community of peasant cultivators who constituted 15 percent of the district's population and generally owned rather than rented the land they tilled. Among Hardiman's major findings are: that the

main village activists were middle peasants (holding fewer than fifteen acres) rather than rich peasants, although rich peasant participation was necessary for a tax boycott to succeed; that elite landlords, bankers, and entrepreneurs were reluctant to risk their material interests by defying the colonial government; and that villages dominated by other castes, Muslims, or landlords usually refused to join the no-tax movements. In other words, the core of the nationalist movement in Kheda District was the Patidar community rather than an economic class. But once relatively affluent Patidar peasants had successfully challenged the government, poorer peasants joined the movement and became more openly revolutionary, withholding rents and feudal services and "attacking the various local representatives of the colonial state" (p. 255).

Hardiman also shows that the Patidars had their own agenda that often clashed with Gandhi's priorities and moderate approach. Gandhi's efforts to recruit soldiers for the British during World War I, his untouchable uplift campaign, his suspension of noncooperation following the Chauri Chaura killing of policemen in 1922, and his 1931 pact with Lord Irwin and consequent cancellation of civil disobedience each alienated many Patidars. When Gandhi chose to boycott the salt tax in 1930, Vallabhbhai Patel and the Kheda Patidars insisted upon refusing to pay their land taxes as well. One-fifth of the district's villages held back their land revenue and over fourteen thousand migrated to Baroda State to avoid government reprisals.

Hardiman believes that the specific examples of the solidarity, activism, and independent initiatives among Kheda Patidars discredits two general theories about how Gandhi and the Congress movement recruited peasants. He argues that the Kheda example shows that mobilization of peasants was community-wide and not "vertical" enlistment of lower-caste clients by patrons or faction leaders. He also discounts the theory that appeals to peasant religious beliefs were primary in recruiting peasants, although he does demonstrate that devotional songs and pilgrim centers aided nationalist communication.

Kheda District ranks with Surat and Midnapur among the Indian districts with the most prolonged, stubborn peasant opposition to the colonial government. Yet how "nationalist" was the peasantry of Kheda? The no-tax campaigns were weak or nonexistent in the many villages dominated by non-Patidars. And when Patidar peasants refused to pay their taxes, how many were trying to end foreign rule and how many were merely seeking lower tax assessments or asserting the community interests of the Patidar caste? Hardiman, who interviewed over one hundred fifty Gujaratis, implies that communal or caste self-assertion was inseparable from anti-

colonialism. He also reminds us that in studying Indian politics, analysis of caste rivalries, structures, and marriage patterns affords understanding that a focus on economic interests and forces is likely to miss.

JOHN R. MCLANE
Northwestern University

CHARLES BACKUS. *The Nan-chao Kingdom and Tang and China's Southwestern Frontier*. (Cambridge Studies in Chinese History, Literature, and Institutions.) New York: Cambridge University Press. 1981. Pp. xii, 224. \$36.00.

This book is in the best tradition of Western studies of China's land frontiers. The southwestern frontier has attracted attention for over a hundred years. Yet, as Charles Backus demonstrates, it is incredible how stubbornly a mistaken interpretation made a century ago is still with us, despite many attempts made to correct it. This erroneous idea is the notion that the Nanchao kingdom was Thai, an interpretation that had led to the rewriting of Thai history. Although it is not the book's primary purpose, it firmly dismisses the idea. But again one wonders whether this will have any effect on those determined to see Nanchao as an ancestor of the Thai kingdom.

The most fascinating thing about Nanchao is that it was the only "empire" to emerge in that area between the Sino-Tibetan world and that of India and Southeast Asia. For a brief moment, its armies reached the South China Sea and tried to reach the Bay of Bengal. But this book shows that, according to the available sources, Nanchao clearly belonged to the Sino-Tibetan world. This is not so much because the core peoples of Nanchao spoke a Tibeto-Burman language but because the kingdom's history was intimately tied to China's relations with the great Tibetan empire of the seventh and eighth centuries A.D. This is disappointing to those who see the area, now modern Yunnan, as an integral part of Southeast Asian history. But as long as the Chinese documents are dominant, that view has to be accepted. Only the possibility of further archaeological discoveries and as yet unknown Indian and Southeast Asian sources leaves some hope for a new perspective.

In the meantime, we have here yet another excellent study of how China traditionally managed its land frontiers. The theme is familiar: wise and brilliant officials, soldiers, and envoys win over the rude and simple "barbarian" kings and chiefs only to have their work ruined by their greedy and devious colleagues. Ultimately, the book's subject is war and diplomacy in the framework of a tributary system devised by the Chinese, which their neigh-

bors had long since learned to use to their advantage. No study can be free from the clichés of Chinese historiography; but with more high-quality studies like this one trying to dig below the surface, a more refined appreciation emerges. We may truly understand what was meant when the poet says, "The Emperor's mind is set on coddling the distant Man" (p. 2).

Although Backus's approach is largely sinological, the author recognizes the limitations of the Chinese sources and uses as much ethnographic, linguistic, and archaeological data as he can find. The book examines critically all earlier studies of the subject and incorporates, after careful sifting, the findings of the latest Chinese and Japanese research. As a work of frontier history, it is authoritative and should remain so for a long time.

WANG GUNGWU
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A. C. MILNER. *Kerajaan: Malay Political Culture on the Eve of Colonial Rule*. (Monographs of the Association for Asian Studies, number 40.) Tucson: University of Arizona Press, for the Association for Asian Studies, Tucson. 1982. Pp. xxiii, 178. \$11.95.

Looking at it from the outside, nineteenth-century observers were able to identify a single "Malay world," possessing common characteristics of language, dress, manners, religion, and custom as well as similar patterns of settlement and occupation. Yet, although among its peoples there may always have been some awareness of cultural unity, the Malay world never achieved an overall political unity. Rather, it was a world of fragmentation and fluidity with the numerous rajas, shifting capitals, vague boundaries, and migrating populations. Governmental and legal structures were extremely loose; the Malay word "kerajaan," often translated as "government," "state," or "kingdom," connoted little more than "being in the condition of having a raja."

European observers generally tended to concentrate on the study of Malay institutions rather than on the concepts that lay behind them and to interpret and evaluate those institutions in Western terms. A. C. Milner, the author of this interesting "case study in political culture," has turned to Malay sources such as the *hikayat* or historical literature in an effort to discern the real character of Malay political motivation, to discover "the way in which Malays made sense of their political activity." He focuses his analysis on two different situations in the Malay world of the nineteenth century: on the rise and expansion of Deli on the east coast of Sumatra and on the Pahang civil war in the Malay Peninsula. The *Hikayat Deli* and the *Hikayat Pahang* are used as

"evidence of the preoccupations and categories of Malay political culture."

An economic interpretation of Malay political motivation came naturally to the European observer. He saw, for example, the quest for control of the East Sumatran export trade, particularly the pepper trade, as the underlying motive of political action in that region. But, as the author of this study is concerned to demonstrate, the Malay raja was interested less in commerce than in personal wealth, because wealth was a means to power and power was reflected and embodied in the size of his personal following and the number of his subjects. Since any wealthy Malay might be a potential rival for power, the raja sought not only to increase his own wealth but also to prevent the accumulation of wealth on the part of his subjects.

Wealth was the key to the raja's power because it could buy the loyalty of leading subjects through the conferment of titles, dignities, and privileges and through the maintenance of costly ceremonial, celebration, and entertainment. Thus, it was wholly in the Malay tradition of *kerajaan* that the rajaship should be conventionalized and formalized and that the raja should be associated more with rites and ceremonies than with the task of actual rule.

All this was noted by European observers, who tended to brand the system as "oriental despotism." They saw clearly that the Malay *kerajaan* did not conform to Western political concepts or categories, but they could not understand why. It was in fact less a political than a magicoreligious system.

This valuable contribution shows what can be done when an enterprising scholar opens up new lines of investigation into previously worked materials and areas of study.

BRIAN HARRISON
St. Paul's Bay, Malta

RAJESWARY AMPALAVANAR. *The Indian Minority and Political Change in Malaya, 1945-1957*. (East Asian Historical Monographs.) New York: Oxford University Press. 1981. Pp. xvi, 260. \$34.50.

This book confines its scrutiny of Malayan Indian politics to the British interregnum between the Japanese occupation and the achievement of full independence. It covers in detail ground already traversed in part by K. S. Sandhu, S. Arasaratnam, Michael Stenson, K. J. Ratnam, Charles Gamba, and others, and it constitutes a useful and worthwhile contribution to Oxford's "East Asian Historical Monograph" series, which has done so much in recent years to publish the work of young scholars of Southeast Asia and to enlarge and enliven our knowledge of the region.

The story Rajeswary Ampalavanar tells is that of

the Malayan Indian community's jockeying for political position in a multiracial society when the realization grew that independence from Britain was inevitable. As the smallest and least powerful ethnic group, they were understandably fearful of all change. British rule had made it possible for the Indian community to remain politically focused on developments in the subcontinent rather than the peninsula, but the movement toward independence for Malaya made it imperative for the Indian minority to shift their concentration of political interest to the place where they lived from the place whence they had come. This was a difficult and often painful process and one in which there were stops and starts, schisms and shifts, debate and acrimony. It is this process that the book describes and that it does much to explicate.

A reiterated theme of the book is the multiplicity of divisions within the so-called Indian community of Malaya. Although smaller than the Malay and Chinese communities, it was even more fragmented. (Despite diverse geographical origins, the Malays were at least united in their religion and their language; the Chinese, despite linguistic diversity and differential assimilation to Nanyang customs, were at least united in cultural pride and economic success.) The Indian community was divided religiously and linguistically, by race and by caste, and by the region of origin. The sixth chapter of the book is perhaps the most illuminating one, as it traces what was going on politically *within* the Indian community during the period under review. It was this internal political activity that to a large extent determined the way in which the community perceived and related to external political events and developments. To an outsider who has had difficulty understanding some of the shifts in Malayan Indian politics of the time, it is reassuring that Ampalavanar confirms the feeling that personality clashes were often as important in determining political strategy or goals as any more objective criteria.

From its inception in 1946 the Malayan Indian Congress has been the single most important vehicle for Malayan Indian political activity. Ampalavanar makes the interesting point that, although it is a democratic body at the grass-roots level, centrally it is highly autocratic. In terms of its constitution the only check on the powers of the party president is the party's so-called Annual Session, and successive presidents have not hesitated to postpone its meetings indefinitely if they feared its strictures. Within the party from the beginning were both radical and conservative wings, so that pre-existing ethnic or class conflicts were frequently exacerbated, but in the final analysis disputants had to realize that in a multiracial society Indian divisiveness harmed no one but themselves.

As the 1940s drew to a close, they had to realize as well that unless they took the business of Malayan politics seriously they ran the risk of being treated as Indian nationals were being treated by the newly independent states of Burma and Ceylon—that is, they would be repatriated to India. The president of the Selangor branch of the Malayan Indian Congress realistically summed up the imperatives: "We cannot fly foreign flags, shout foreign slogans, sing foreign national anthems, give our loyalties to a foreign country, and at the same time want to meddle in the affairs of Malaya. To continue to do so is to arouse the hostility of the Malayan people" (p. 157).

In the early 1950s the Malayan Indian Congress joined in electoral alliance with the United Malays National Organization and the Malayan Chinese Association, and despite vicissitudes this has continued to the present. This coming together with other communally based political parties caused the Malayan Indian Congress to move towards the center of the political spectrum and to lose both working-class votes and some of its more colorful and vigorous leaders. Within the Alliance, and later within the *Barisan Nasional*, the Malayan Indian Congress has been a very junior partner, both in terms of its general political leverage and in terms of its ability to deliver the votes of the community it purports to represent.

Ampalavanar's thematic organization of chapters necessarily leads to some repetition, but this is not excessive. And she does much to clarify a confusing and turbulent period in the history of the Indian community of peninsular Malaysia.

MARGARET CLARK

Victoria University of Wellington

HUỖNH KIM KHÁNH. *Vietnamese Communism, 1925–1945*. Ithaca, N.Y.: Cornell University Press, for the Institute of Southeast Asian Studies, Singapore. 1982. Pp. 379. \$25.00.

Huỳnh Kim Khánh's *Vietnamese Communism, 1925–45* fills an important gap in the history of the origins of the Vietnamese Communist movement. It nicely complements David Marr's newly published *Vietnamese Tradition On Trial, 1920–1945* (1981), which concentrates on the cultural crisis of those years. And it provides more detail on how the Communists succeeded in "grafting . . . Marxism-Leninism onto the stock of Vietnamese patriotism" (p. 341) than was possible in general histories of the Communists by myself, William Duiker, Douglas Pike, Robert Turner, and others.

There are especially useful details—based on extensive Vietnamese sources—on the factional divisions among the Vietnamese after 1925, on the

effects of French colonialism and internal politics on Vietnam, on the effects of the Japanese (and Thai) invasions after 1940, on the Communists' work with the nationalities, on the development of the armed forces, on the famine of 1944–45, and on the relations between the Communist International and the Communist party of Indochina, the party's official name during those years.

Regarding the early and mid-1930s, for example, Milton Sacks and Daniel Hémery earlier provided considerable information on the surprising collaboration of Vietnamese Trotskyists and Stalinists in Saigon, but Khánh's materials make the episode all the more astonishing. Thus, for several years after 1933 the Vietnamese Stalinists simultaneously condemned the allegedly excessive nationalism of Ho Chi Minh (called throughout the book by his most famous early pseudonym, Nguyen Ai Quoc) as a "gross error . . . a vestige of petit bourgeois ideology" (p. 179), but at the same time collaborated with the Vietnamese Trotskyists, who of course were even then attacking Stalin's "petit bourgeois" and very nationalist policy of "socialism in one country." This anomaly grew out of the extreme conditions faced by the Vietnamese revolutionaries, their common bonds of friendship, and their common enemy. The collaboration finally ended when conditions changed with the outbreak of World War II.

Khánh is less successful in dealing with the tricky relationship between nationalism and communism in Vietnam. The book is correctly based on the premise that the Communists' success in and after 1945 was possible because of their, and especially Ho Chi Minh's, success in becoming the "legitimate representatives of Vietnamese national interests" (p. 20). And in a brief epilogue he correctly poses the ultimate question for the future of the Communist revolution, can Marxism-Leninism continue to be the "vehicle for Vietnamese patriotism" it was before 1975 when its tasks were far different (p. 341)?

But he is on less sure ground on the one hand in dealing with the failures of moderate non-Communist nationalists, whom he accuses of being mostly "collaborationists" (p. 39), and on the other hand, with the betrayals of Vietnamese nationalism by the "Internationalist" Russian returned students, who directed the party in the 1930s. If the ultimate question of nationalism is how to improve and strengthen your country, then both moderates willing temporarily to work with France and the United States or extremists believing only immediate world revolution could "save" their country can claim to be nationalists, whether conservative or revolutionary.

What enabled the Communists alone to capture and effectively lead Vietnamese nationalism was the dedication and brilliance of their organizational work, through which they were able to mobilize and recruit key sections of the intelligentsia, peasants,

workers, and minorities. The author notes the "centrality" (pp. 77, 334) and uniqueness relative to other Vietnamese parties of Communist organizational, mass, and minority work, as in combating the terrible famine of 1944–45, but would have done better to make discussion of that organizational work more a feature of the book. He then might have avoided some over-simple sentences, such as that beginning his second paragraph, which states that Communist "success, paradoxically, was possible largely because of the efforts of France and the United States to destroy the Communist Movement in Vietnam" (p. 19).

It may be true that had neither France nor the United States tried to stop them, the Communists would not have come to power in Vietnam—an idea Washington would do well to ponder. But it is equally true that only the dedication and resilience of the Vietnamese Communists, rooted in their ideology, organization, and work with the masses, enabled them to withstand the awesome pacification efforts of France and the United States. This book makes an important contribution to clarifying the early development of that commitment and achievement.

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UNITED STATES

JAMES MACGREGOR BURNS. *The American Experiment: The Vineyard of Liberty*. New York: Alfred A. Knopf. 1982. Pp. xii, 741. \$22.95.

This volume is the first of a projected trilogy that will examine *The American Experiment* from the framing of the Constitution to the present. The author, James MacGregor Burns, a distinguished political scientist and prize-winning biographer of Franklin D. Roosevelt and John F. Kennedy, brings to his formidable task a very consciously used literary talent and the idealistic moral concerns of a liberal political activist. Handsomely produced by Alfred A. Knopf, *The Vineyard of Liberty* will provide few revelations of a substantive kind for academic historians, but it merits their attention because of its style and its argument.

Read on one level, it is a narrative of American development starting with Shays's Rebellion and ending quite abruptly with the Emancipation Proclamation. Most of the topics, events, and personalities conventionally dealt with in college texts are covered, with a reasonably good balance between the central political theme and relevant social, economic, and cultural trends. In a work of such broad compass, one can disagree with Burns's sense of proportion—in my judgment his treatment of such

critical events in the politics of slavery as the Missouri Compromises, the Wilmot Proviso, and the Kansas-Nebraska Act was less than adequate—but there is surely room for latitude here.

What is most striking about the book is its rhetoric; it departs markedly from the genre familiar to readers of the *American Historical Review*. Burns determinedly seeks to make his account lively, dramatic, engaging. Accordingly, he provides graphic settings for important scenes. As the new federal government began its existence in New York City, "Hogs rooted through the garbage-clogged gutters, cows wandered up and down Broadway, horses hooves clattered on the cobbled streets" (pp. 76–77). There are vivid etchings even of minor characters, such as Amos Kendall, "nearsighted, asthmatic, prematurely white-haired, bundled up in a white great-coat even on a blazing hot day" (p. 326). Sweeping characterizations are framed with snappy wit; the Founding Fathers were "the well-bred, the well-fed, the well-read, and the well-wed" (p. 33). Even the chapter titles—"The Wind from the West," "The Majority that Never Was," "The Ripening Vineyard," "The Blood-Red Wine"—are evocative literary devices. Frequently the narrative adopts the form of swiftly changing scenes, much like a television drama, or is interrupted for the depiction of little-known but colorful characters. Whether such artfully contrived efforts to "bring history alive" will be appreciated by academic purists is doubtful, but if history is to reach out to a larger public, this form of communication may well be effective.

A bold interpretation, coupled with decided judgments, shapes even though it does not entirely integrate Burns's narrative. As he sees it, the American republic was an experiment whose success was to depend on its ability to ensure liberty and equality for all the people. The framers, led by Madison, designed an intricate federal political system featuring majority rule, minority rights, balanced representation, separation of powers, and checks and balances. This polity, even in the best of times, required for its successful operation a vast system of brokerage, but it could surmount recurring crises only through the intervention of masterful leadership.

In time, "the people's constitution" added the instrumentalities of mass political parties, thereby creating at least the potential for effective majority rule and, presumably, for the achievement of liberty and equality. But these transcendent values were never clearly defined, they remained vague, and masterful leadership was sadly lacking. Even Jefferson, Jackson, and Lincoln were flawed. The new popular parties that emerged in the 1830s did not formulate coherent political philosophies, and "the majority that never was"—women, blacks, Indians, the poor—failed to mobilize its political power.

Burns cannot conceal his disappointment that a party never formed "that could provide the nation with a radical, democratic, popular national government that might . . . unite the separated organs of government behind a comprehensive national effort to meet the needs and aspirations of the great number of lower-income people" (p. 382). As the political crisis of the 1850s worsened, no towering leader arose to clarify the issues and point the way toward their resolution. Neither in the North nor in the South was the value of liberty treated analytically or linked to the value of equality. The overall consequence, then, was that the vineyard failed to produce its fruit; the experiment met with the disaster that was the Civil War.

Obviously, this interpretation is open to challenges, though not in this space. One can only observe here Burns's surprising failure to confront head-on, as he might well have done, Madisonian pluralism and its implications. Instead of condemning "the system," he deplores the lack of the singular leadership that was required to surmount the defects in the system. It is disconcerting, too, that he does not account sufficiently for the centrality of the ideas of liberty and equality in his vision of America, nor does he attempt to relate these concepts to the larger model of "republicanism" that is currently emerging in our scholarship.

Other caveats could be entered, but one is still impressed by the grandeur of the enterprise, the splendor of the prose, the passionate concern that informs the judgments. This may not conform to the varied prescriptions of Lawrence Stone or Bernard Bailyn for a new narrative history, but it is an American history with style, energy, and moral commitment.

RICHARD P. MCCORMICK
Rutgers University

RICHARD P. MCCORMICK. *The Presidential Game: The Origins of American Presidential Politics*. New York: Oxford University Press. 1982. Pp. 279. \$19.95.

Although the American political system is preeminently a presidential system, appropriate methods for selecting the chief executive stymied the framers of the constitution and puzzled later generations. The framers' solutions quickly revealed serious flaws. In the half century following the adoption of the federal constitution, Richard P. McCormick argues in this succinct and well-written book, "the presidential game," that is, the presidential selection process, went through four systems before achieving stability. The triumph in the 1840s of "the party game" established the rules that prevailed until the second half of the twentieth century when the decline of parties stimulated the search for new

rules. Whether a democratic political system can survive the recent trends toward plebiscitarianism and a system of nominating candidates in which money and personalities, buzz words and media hype become central, debase the process of rational choice, and limit opportunities for mass participation that party politics allowed are a few of the unsettling implications of McCormick's valuable epilogue that deftly traces the history of the selection process since the period when it took its classic form.

The founders designed the presidency on the assumption that "exalted characters" like George Washington would be chosen to head the executive. A George Washington, however, appears perhaps once in the life of a nation and even before he left office, Americans refused to play the presidential game as the framers intended. The emergence of parties in the 1790s, McCormick persuasively contends, guaranteed that politicians, not suprapartisan spirits, would compete. Partisan maneuvering quickly exploited the hazardous and uncertain procedures inherent in the framers' design. Not until the 1830s was there a uniform method of choosing presidential electors on statewide party tickets rather than by state legislatures or by the voters in districts. Both democratic ideology and party interest—the chance to capture a state's entire electoral vote—assured the triumph of the general ticket. Likewise the anomaly of 1796 when president and vice-president came from different parties and the succession crisis of 1801—"a near disaster"—illustrate the hazardous character of the process.

Parties, McCormick maintains, came to the rescue. The pursuit of partisan interests produced the Twelfth Amendment assuring that the victorious party captured both top executive offices. Partisans also tended to favor popular election of electors on statewide tickets and they created the legislative caucus to nominate the party's standard-bearers. These revisions of the rules produced "the Virginia system" under which that state dominated the selection of three presidents. By the 1820s, however, Virginia had no pre-eminent candidate nor did it still enjoy the political weight that earlier allowed it to dominate the system. From 1824 to 1832 a factional system prevailed, characterized by personal rivalries, vicious infighting, and constant intrigue. Yet factional leaders, having destroyed the caucus method of nominating, searched for ways to legitimize their candidacies. Mass conventions, new popular campaign techniques that increased voter participation, the extension of the general ticket, and the advocacy of issues sketched the rules of the next stage, rules perfected in "the party game." By the 1830s faction leaders were evolving into party leaders, and "the presidential game had become the party game," a system every element of which "was at variance with the republican ideology that had shaped the action of the founders when they con-

trived their process of presidential selection" (p. 205).

This book is an admirable example of how a skillful historian can pour old wine into new bottles and produce a stimulating new vintage. McCormick's data are largely familiar, but he extracts fresh insight from them. In a previous work, McCormick demonstrated how the struggle for the presidency stimulated party development in the age of Jackson. Now he shows persuasively how the parties transformed an often impractical and hazardous system of presidential selection into a more predictable, more open and democratic process than the framers had devised, albeit not immune from "the mischiefs of faction."

PAUL GOODMAN
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Davis

RICHARD R. JOHNSON. *Adjustment to Empire: The New England Colonies, 1675–1715*. New Brunswick, N.J.: Rutgers University Press. 1981. Pp. xx, 470. \$30.00.

Richard R. Johnson respects those studies that have explored "the New England Way" through religious and social developments but denies they aid understanding the most significant changes of the era of the Glorious Revolution. These changes arose from the intrusive forces of the mother country to which the region's colonists were obliged to adjust.

Johnson singles out strong leadership and institutional reform as the primary instruments of a successful response. With the Bay Colony in the lead and guided by politicians reconciled to the realities of English power yet resolved to preserve essential traditions, New England escaped the ashes to which the centralizing schemes of the Stuarts had seemed to confine it. Reorganization of local administrations, including the judiciary, was important. The seaboard merchants had a crucial role. Aware of opportunities within an Atlantic community, not until the 1690s did they operate effectively as members of the provincial council—so Johnson argues in opposition to Bernard Bailyn. But by 1715 they had learned successfully to make politics "a continuation of business" (p. 415). Supported by leading dissenters such as Sir Henry Ashurst, Massachusetts cautiously demonstrated how ties with Whitehall could be regularized and exploited.

Examination of the common Anglo-American social and intellectual heritage has in this study produced meager fruit. One looks in vain for recognition of what Malcolm Morrison has called an "intellectual triangular trade" among the Nonconformists in England, Scotland, and New England. Scotland gains no mention, though its impact on the process of adjustment was considerable. Johnson's unconcern with the Act of Union (1707) is else-

where evident in his reference to New England's clamor for the protection of "British" frigates at a time when the Royal Navy was still as self-consciously English as the navigation laws. Scholarly resources, primary and secondary, relating to such matters as manning and convoying ships, and the deployment of provincial and metropolitan forces—issues over which vital compromises took place—are virtually ignored. Johnson is notably patchy in his awareness of those who have charted or trodden the path before him.

Acceptance of constitutionalism, the ideological framework of the relationship with the mother country, is assumed to imply rejection of the crown. Unlike Perry Miller, who stressed the importance of the Protestant monarchy to New Englanders, Johnson ignores its personal face. Yet what he means by the crown is not always clear. Beginning his final chapter with a quotation from Cotton Mather's "Pillar of Gratitude" (central to this reviewer's work though not so acknowledged), Johnson passes from nation to crown as though they were one and the same thing whereas the latter was no more than one variable in the complex expression of national sentiment. What this curious conflation emphasizes, in an otherwise highly valuable study of politics, is the author's preoccupation with New England's elite. That the feelings of lesser mortals may have significantly influenced the process of adjustment is scarcely reflected on.

PHILIP S. HAFFENDEN
University of Southampton

JAMES H. LEVITT. *For Want of Trade: Shipping and the New Jersey Ports, 1680–1783*. (Collections of the New Jersey Historical Society, number 17.) Newark: The Society. 1981. Pp. xi, 224. \$21.45.

This monograph on New Jersey commerce illustrates both the strengths and shortcomings of the quantitative approach to historical study. The precision of quantitative method is offset by the state of the records that supply the data base. The New Jersey records have deplorable gaps, as James H. Levitt explains before claiming, properly in my judgment, that these lacunae do not seriously impair their value for the quantifier. Exactly how far computer analysis has dictated the structure of his work I find difficult to determine, but ironically those pages that speak most clearly and effectively rest on traditional research and time-honored methods of case study or biographical sketches. Whatever the case, by combining traditional and social science techniques the author fills a void in the history of colonial New Jersey left by contemporary scholarship with its political and institutional orientation.

Every student of early American history knows the jocose observation that the New Jersey economy

resembled a barrel tapped at both ends, by New York at one end and Philadelphia at the other. In search of an explanation for this state of affairs, Levitt develops an interesting argument. New Jersey, he holds, failed in the face of rivals on either side because service support—shipwrights, ropewalks, sailmakers, insurance underwriters, along with merchants themselves—never grew to appreciable dimensions (pp. 7–8, 119, 138). This complex of merchants and allied services failed to evolve because the provincial assembly of New Jersey, dominated by agrarian interests, would not and did not pass legislation to ensure its growth. Though plausible, this hypothesis appears simplistic, certainly when compared with the factor analysis of Jacob Price (*Perspectives in American History*, 8 [1974]). Nor does it explain why Perth Amboy, the leading New Jersey port, had a respectable and increasing commerce that peaked in the mid-1720s and declined thereafter.

To my mind the author has greatest success with his descriptive chapters. Chapter 3, "Port Administration," includes sketches of two collectors illustrating port practices and personnel. Chapter 4, "Patterns of Trade," for the first time defines the direction of New Jersey commerce, rather heavily bilateral, with New England, along the coast, and increasingly with the Southern colonies (data in table 4). Chapter 5, "Vessels, Crews, and Cargoes," presents information, often omitted and not readily accessible, on sizes of vessels and crews, the age, construction, and design of carriers, and finally on shipmasters and sailors. Another meaty section, chapter 6, "Merchant-Shippers," in addition to elaborating the author's argument that New Jersey ports fell behind for failure to attain that commercial complex essential to ultimate success, also deals with such tangibles as merchant-shippers, their agents, partners, bookkeeping methods, all neatly illustrated with brief case studies.

In short, following the author's stated premise that local studies contribute most to understanding early American development, these chapters and selected tables provide a wealth of detail on the hitherto neglected New Jersey ports. Regrettably, Levitt fell into the slough of ungraceful writing. Minimum care could have eliminated the personalizing possessives ("New Jersey's ports") that have become a disease, at once unclear and grating to the ear.

AUBREY C. LAND
University of Georgia

A. ROGER EKIRCH. *"Poor Carolina": Politics and Society in Colonial North Carolina, 1729–1776*. Chapel Hill: University of North Carolina Press. 1981. Pp. xix, 305. \$23.50.

The theme of this very good book is: "If poor countries appear to be unstable, it is not because they are poor, but because they are trying to become rich" (p. 219). North Carolina in the late colonial period, 1729–1776, was poor, politically unstable, yet moving toward cohesion. It was the "Best poor mans Cuntry" (p. 28) because of the great amount of land waiting to be possessed and used.

How did men obtain land? How did they climb into the emerging elite? Influence, patronage, and corruption were the three engines of eighteenth-century advancement. Lord Granville, the only proprietor not to sell to the crown in 1729, obtained in 1744 by a grant from the king in council his Granville district. He and Henry McCulloh were the two greatest landholders in North Carolina, and a distinct virtue of this book is to show not only the unsettling effect of their operations but also how the results were ultimately woven into the fabric of the colony's social history. Other worlds of patronage swirled around each royal governor: George Burrington, Gabriel Johnston, Arthur Dobbs, William Tryon, and Josiah Martin. A. Roger Ekirch offers a theory of corruption. "Corruption can be functional" (p. 160); "corruption in North Carolina probably served a vital function by contributing to political integration through the creation of bonds of mutual interest" (p. 159).

There were regional differences, North versus South early and East versus West later, but these were never class struggles. The members of all of these groups had the same ends in view. The beauty of the book is the care with which these stories are told. There were two crucial ones, one of the Granville tract and the other of the Regulators. The first has not often been told and seldom as clearly as here. The latter has been told often in recent years, but no one has summed up the Regulator movement in North Carolina with more sense and understanding. After a succinct summary of the historiography of the Regulator movement, Ekirch explains that the movement was a manifestation of the fact that the backcountry society was a more fluid society, filled with nouveaux riches, men who had come from other colonies. The very rapid influx of men from Pennsylvania meant that these new men did not know their superiors, had no extensive family ties, but like the men on the coast who had already partially succeeded were "Knaves alike" (p. 192).

In "Patriotism Unmasked" (p. 112) he shows that the lowcountry society used the rhetoric of free men to fight the corruption of England, yet a little corruption had helped them along. In the Regulation there was the same push. There was also rhetoric, but it was of the country party and principles. Thus the stance of North Carolina in 1776 was a combination of interest and principle. No one could tear them apart and see them separately.

This sophisticated account rings true in so far as it goes. But the story could be fleshed out by comparing the situation in North Carolina with that in other colonies. For example, the struggle over blank patents in North Carolina was similar to the struggle over proprietary patents in South Carolina.

But even more we need to go behind the scenes in England to examine the ultimate springs of influence. There are references to these, but the author does not tell us how Granville and McCulloh's friends at home were intertwined or how the men who sponsored the various royal governors in turn were connected to each other. James A. Henretta in "*Salutary Neglect*," *Colonial Administration under the Duke of Newcastle* (1972) has told us that these connections were important, and Ekirch has provided additional tantalizing glimpses, but there is still much to be done. Then we would see better how a McCulloh could strut in Carolina and why eventually separation from England had to come. But it came only after many of the men had profited from the old system. Having climbed to the heights, they wanted to sweep the ladder from behind them and disown the much bolder and more corrupt system at home in order to begin a new order in America.

GEORGE C. ROGERS, JR.
University of South Carolina

RHYS ISAAC. *The Transformation of Virginia, 1740–1790*. Chapel Hill: University of North Carolina Press, for the Institute of Early American History and Culture, Williamsburg, Va. 1982. Pp. xxxii, 451. \$29.50.

A simple argument lies at the center of Rhys Isaac's gracefully written evocation of eighteenth-century Virginia culture. A society integrated around gentry leadership in 1740 divided into conflicting cultures by 1790 as Baptists and Methodists won adherents among middling and lesser planters, slaves, and a sprinkling of gentry. Jefferson's 1786 Act for Establishing Freedom of Religion reduced the established church to a mere denomination, followed soon thereafter by the reduction of vestry powers, and by the physical deterioration of church buildings. Beyond the institutional changes, evangelical religion brought virtually the whole of gentry culture to judgment and advanced a counterculture of its own. Dancing, for example, an art at which the gentry excelled and that joined them culturally to the lower sorts, was proscribed, and with it the entire assertive, competitive gentry style. Evangelical conversion introduced people to an austere but warmly emotional brotherhood of equals. In perhaps the most provocative suggestion in a book overflowing with provocative ideas, Isaac portrays the Baptist challenge to the establishment as a cultural confrontation. The Bap-

tist movement, he writes, is best understood "as a rejection of the style of life for which the gentry set the pattern, and as a search for different models of proper conduct" (p. 172).

But as the book progresses the simple argument with its elegant elaborations becomes more complex and ironic. Isaac's concern with religion arises from an interest in the cultural and social position of the middling and lower sorts. Evangelical religion enabled them to set themselves apart from the gentry culture to which they were only weakly attached anyway. But there were other modes of detachment. The Baptists and Methodists were not the sole challenges to the traditional order. Revolution and republicanism introduced strains of their own, apart from and contrary to evangelicalism. Individualism and contractual society, Isaac argues, were the fruits of republicanism, undermining patriarchy and communalism. Evangelicalism was individualistic in the lonely search for salvation and in the opposition to a religious establishment, but evangelical worship was deeply communal. In the Virginia of 1790, individualistic political influences pulled one way, and evangelical religion another.

The complexities multiply as Isaac enlarges the picture. The book reminds us, for example, that the gentry were as critical of the established clergy as the evangelicals. Baptist complaints of unfeeling Anglican preaching were no more harsh than the great planters' charges of lax discipline and clerical greed. Ironically, the Baptist movement flourished in the very decades when Richard Bland and Samuel Henley were leading the gentry in opposition to an Anglican bishop and improved clerical salaries. The convergence of the two impulses casts doubt on the strength of the Baptist challenge. Would the gentry have dared attack the Anglican clergy so vigorously if the Baptists were a serious threat?

The Transformation of Virginia recasts Virginia history in the last half of the eighteenth century by highlighting instability and change, as, for example, Charles Sydnor's *Gentlemen Freeholders* did not. But the book may not be valued in the long run for its explanation of why Virginia changed. The disturbing effect of the evangelicals seems somewhat exaggerated. Isaac himself gives weight to other influences in the conclusion. Cosmopolitan gentility, only lightly touched on earlier, is assigned nearly as large a part as dissenting religion in the description of the new society. Individualism looms larger still, and its origins are only partially elucidated in the body of the book. The chapter on transformed Virginia is diffuse as well as complex.

The concluding "Discourse on Method" offers a systematic approach to historical ethnography, using episodes in diaries and travel accounts as field notes, and recommending a dramaturgical mode of analysis. We can be grateful to Isaac for attempting

to conceptualize his techniques, but obviously his gift for interpreting incident and scene contribute more to the success of the book than any system. He magically transforms the piedmont militia companies' hunting shirts into a potent symbol of the people in arms, set against the buff and blue uniforms of the gentry companies. His walk through the Virginia woods in the boots of gentry and common planter, and then barefoot as a slave, is a tour de force of historical imagination. The analysis of the appointment of the Bruton Parish rector in 1772 and of the return of one of Landon Carter's runaway slaves reveals layers of meaning invisible to most eyes. *The Transformation of Virginia* will be valued most for these insights. In the early chapters where the explications are sometimes perfunctory and the argument occasionally flags, reading grows tedious. But by the end, the book convinces us that close attention to commonplace events and their settings by someone of Isaac's ability will give us fresh access to long lost worlds.

RICHARD L. BUSHMAN
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JAY FLIEGELMAN. *Prodigals and Pilgrims: The American Revolution against Patriarchal Authority, 1750-1800*. New York: Cambridge University Press. 1982. Pp. vii, 328. \$24.95.

This important book establishes the place of the American family in the American Revolution. Others have demonstrated that a familial vocabulary was the lingua franca of the Revolution. Jay Fliegelman traces that lingua franca to writings about the family itself. The most widely read books in the colonies concerned family relations, he shows, and he analyzes those books brilliantly and in detail. Locke's *Some Thoughts on Education* (rather than his *Second Treatise*) becomes the most significant text of the American Enlightenment. As the colonies declared their independence from parental tyranny and moral corruption, so had a generation of sentimental heroes and heroines done before them. Domestic emancipation prepared the way for political freedom.

The American revolution against patriarchal authority did not destroy the family. Instead, the patriarchal family—hierarchical, distant, and governed by deference and force—gave way to a more intimate, egalitarian, natural family. Whether the infant was seen as a *tabula rasa* (in the Lockean version) or as naturally social and affectionate (in the Rousseauian and Scottish common sense view), he was formed by parental influence. But the confidence in infant nature and parental nurture had a dark side. The ideology of the natural family blamed parents for the failings of children. It made

innocent, malleable children susceptible to bad influence, to worldly deception and corruption. And it undermined the very distinction basic to its world view, that between corrupt methods of influence and benign ones, between seduction and love.

Prodigals and Pilgrims dissolves the opposition between those who see reason and natural right in the Revolutionary mentality, and those who stress projection and paranoia. Independence and anxiety, this book suggests, were two sides of one coin. Nonetheless, Fliegelman's Revolution is an optimistic moment in the history of the natural family. Like other recent familiarly based interpretations of American history, *Prodigals and Pilgrims* swallows up all of society in (real and metaphoric) kinship ties. Connecting familial language to political factionalism, market conflict, and chattel slavery might have exposed other tensions internal to America and the extrusion of bad influence on monstrous mother England and the pretended father king.

The last portion of *Prodigals and Pilgrims* charts the post-Revolutionary return of anxiety to America. The new national family, with Washington as father, operated on Enlightenment principles of affection and example. Washington did not reproduce the patriarchal domination of George III. But the new familial bonds generated a claustrophobia of their own. On the one hand, the outside world became ever more sinister, promoting the nineteenth-century retreat to walled interiors—the family cut off from society, America cut off from the world. On the other hand, gratitude, emotional indebtedness, and guilt bound children to their virtuous parents. Yet sons were urged to independence; Washington was their model. An edition of *The New England Primer* changed the verse representing the letter "W" from "Whales in the sea, God's voice obey," to "By Washington great deeds were done" (pp. 209–10). Children told to imitate Washington yet confined within the moralized family were placed in a bind. The stage was being set for an Ahab.

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JAN WILLEM SCHULTE NORDHOLT. *The Dutch Republic and American Independence*. Translated by HERBERT H. ROWEN. Chapel Hill: University of North Carolina Press. 1982. Pp. xii, 351. \$29.95.

No more appropriate contribution to the bicentennial observance of the recognition of the United States by the Netherlands has been forthcoming than this insightful book from the pen of a leading scholar of Dutch-American relations. It has, perhaps, remained for Jan Willem Schulte Nordholt to

fulfill the prophecy made by John Adams in 1823 that the Dutch support of the United States in the American Revolution, for so long given little weight, would some day be shown to be "of more importance" (p. xiii).

While touching a good many of the traditional bases, the author does not give us a conventional diplomatic history. Rather, he tells the tale of how the Dutch involvement in the American Revolution prompted the radical change in Dutch politics and society that convulsed the Netherlands in the decade of the 1780s. A penetrating observer of Dutch national character, Schulte Nordholt catches the mood of the era—Dutch money-mindedness, that pervasive sense of the nation's decline as an international power, a people divided by cabal and faction, and a republic dying of its own impotence. Indeed, as the author points out, it was not for lack of understanding that the American Founding Fathers rejected Dutch-type federalism springing from the Union of Utrecht of 1579, but preferred instead to establish a more centralized federal system.

While the roles of Dutch smuggling, gunpowder supplies, and financial aid to the American insurrectionaries are appropriately treated, the gist of the book is a series of skillful pen portraits of leading and secondary actors—of the timid conservative William V, out of his depth in the role of stadholder, of such pro-Americans as the controversial Jean Derk van der Capellen, egalitarian, Commonwealthman, and the first person in the United Provinces to come out openly for the American cause, only to be baffled when the Americans failed to abolish slavery. We meet in these pages François Adriaan van der Kemp, the Mennonite political preacher of Leyden, and, on the opposing side, the fiery Jewish Orangist, Isaac de Pinto.

In treating the first Dutch ministry to the United States headed by Pieter Johann van Berckel, Schulte Nordholt focuses on the diplomat's own reaction to America, his disappointment at discovering the difference between the vision and the reality. Nowhere will one find the nuts-and-bolts issues that commanded the attention of the Confederation government. For example, there is no account of the Dutch-American conflict over the sloop *Chester*, of the dispute over the form of sea letters, of the Dutch complaints about duties levied on Dutch imports by the separate states, of the Van Staphorst–Maryland loan negotiations, or of the effect of the bankruptcy of the firm of de la Lande and Fynje upon the syndicate of Amsterdam banking firms involved in loans to the United States. For the last we must turn gratefully to Pieter Jan van Winter's classic account of fiscal relations between the two countries, recently available in an English translation.

The star of Schulte Nordholt's drama is John Adams, and the author's judicious evaluation of

Adams's exploits, initiatives, and inventiveness provides a welcome corrective to some of the recent negative reappraisals of the New Englander as diplomat. Perhaps the author's most important contribution is his rescue of Charles Guillaume Frédéric Dumas from undeserved obscurity. Drawing upon Dumas's massive letterbook in the Algemeen Rijksarchief at The Hague, the author tells how that enthusiast for all things American played a conspicuous and constructive role in Dutch-American relations, save for his indiscretions during the Orangist overturn, activities that placed him in mortal peril and temporarily soured U.S.-Dutch relations.

In short, Shulte Nordholt's book in its special area is nonpareil.

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HERBERT J. STORING, editor. *The Complete Anti-Federalist*. Assisted by MURRAY DRY. Volume 1, *What the Anti-Federalists Were For*; volume 2, *Objections of Non-Signers of the Constitution and Major Series of Essays at the Outset*; volume 3, *Pennsylvania*; volume 4, *Massachusetts and New England*; volume 5, *Maryland and Virginia and the South*; volume 6, *New York and Conclusion*; volume 7, *Index*, compiled by JOHN ROBINSON. Chicago: University of Chicago Press. 1981. Pp. xx, 123; xii, 452; xiv, 213; xiv, 287; xii, 308; xiv, 249; viii, 109.

The Constitution of the United States was not negotiated between a checkmated monarch and leaders of a rising bourgeoisie, as so many European constitutions were, but rather within the third estate alone, by representatives of various regional and economic interests and political persuasions. The result of their deliberations was submitted to the sovereign of the republic, the electorate, which gave its approval through the recently invented institution of the ratifying convention. Long before independence Americans had enjoyed a rare degree of political liberty, including a free press, and it was to be expected that dissent as well as approval would be fully and publicly articulated, in the ratifying conventions as well as in a full-fledged public debate in newspapers and pamphlets.

Why did the dissenters reject the draft Constitution, or, more precisely, why did they say they did? Thanks to the late Herbert J. Storing and his former student, Murray Dry, we now have most convenient access to the documentary material relating to Anti-Federalist political ideas. On over seventeen hundred carefully edited, annotated, and superbly indexed pages (bound in seven slim volumes) they have assembled sixty-five anonymous newspaper essays (several of them consisting of lengthy series of

"letters"), over a dozen speeches in ratifying conventions and assemblies, six pamphlets, three resolutions of conventions and ad hoc committees, and two semiprivate letters. No new manuscript material is presented.

The first volume consists of Storing's one-hundred-page interpretation of "What the Anti-Federalists Were For," and the texts of the Articles of Confederation and the Constitution with cross references to the pages in the following volumes where Anti-Federalists discuss the issue regulated by the preceding sentence or paragraph—a very useful service to constitutional scholarship. The second volume presents the letters, speeches, and pamphlets in which the six "non-signers," Elbridge Gerry, George Mason, Robert Yates, John Lansing, Luther Martin, and Edmund Randolph justified their refusal to put their names to the draft Constitution. And it contains the major series of anonymous Anti-Federalist articles that were published in New York and Philadelphia newspapers between September 1787 and April 1788. This volume could even be taken by itself as an intellectually complete *Anti-Federalist*, to serve as the major statement for the opposition. The other volumes organize by states the remaining public statements critical of the draft Constitution.

Herbert Storing's introductory essay summarizes concisely the Anti-Federalist argument without attempting to provoke a scholarly dispute by overstating a thesis. He corrects other scholars' exaggerations. He found, for instance, sufficient awareness in Anti-Federalist writings of the existing economic and political difficulties to reject Forrest McDonald's description of Philadelphians in 1787 as "serenely unaware that historians were one day to know this as the Critical Period of American history" (volume 1, p. 26). Many Anti-Federalists, Storing found, did not ignore the critical situation of the Confederation, but they differed from the Federalists in expecting improvement not from drastic constitutional changes but from specific measures such as nonimportation of European luxury goods and taxation of luxuries produced at home. Storing also corrects Gordon Wood's interpretation of antifederalism as a premodern, preindustrial, preliberal political attitude, characterized by the naive assumption that "some organic common good" or "a single homogeneous interest" had to be shared by all virtuous republicans (volume 1, p. 83). Anti-Federalists were no less liberals than their opponents in the sense that "they see the end of government as the security of individual liberty, not the promotion of virtue or the fostering of some organic common good" (volume 1, p. 83). Because of the breadth and variety of Anti-Federalist arguments, Storing rejects the spelling "Antifederalist": it suggests "more cohesion than actually existed,

while 'anti-Federalist' suggests a merely negative, dependent unity" (vol. 1, p. 79 n. 6).

This is not a documentary history of Anti-Federalist politics and personalities, but it is now the best documentation of Anti-Federalist political thought available. In the great debate of 1787–1788 the Anti-Federalists built on the hope that the American republic need not become a nation state with as dangerously powerful a government as the Old World countries that had surrendered their liberties to arbitrary rulers.

WILLI PAUL ADAMS
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MICHAEL J. HEALE. *The Presidential Quest: Candidates and Images in American Political Culture, 1787–1852*. New York: Longman. 1982. Pp. xi, 268. \$11.95.

Arguing that "the presidential campaign assumed its most glorious dimension in the second quarter of the nineteenth century" (p. vii), Michael J. Heale devotes most of his book to the second party system. The period before 1824 is quickly dealt with in the first chapter, because Heale sees little difference between the presidential candidacies of any of the first six presidents—not even between Jefferson and John Adams. Asserting that theirs was an era in which "gentlemen of standing gravely accepted office as a duty incumbent upon them" (p. 20), Heale invents the term "Mute Tribune" to apply to all the early presidents. He uses this designation because early presidential candidates did not take to the stump or openly electioneer to promote their own candidacies, but it seems an incongruous term in many respects to apply to presidents who were often effective communicators of their ideas and successful spokesmen for the nation.

Heale devotes separate chapters to the elections of 1824 and 1828 and then treats the remaining period as a whole, examining separately the Democratic style in the quest for the White House and the Whig style. This leads to some repetition but enables him to see different characteristics of the two parties continuing over time. He regards Jackson as the last Democratic candidate to achieve office in his personal capacity and sees subsequent Democratic candidates as instruments, rather than leaders, of the party. Democratic candidates became increasingly less visible and the party and its platform more important than the candidate. The Whigs were more given to hullabaloo than the Democrats, held more parades, wore more campaign emblems, and took more to the stump. They too moved away from a leader-oriented party but were less committed to party than the Democrats, resorting to party to resist the idea of party. He concludes that the characteristic pose of Democratic presidential candidates was

one of low visibility and high party commitment; the characteristic stance of Whig candidates, one of high visibility and low party commitment. Whigs sought men of stature, enabling military heroes to be especially successful in winning nomination.

Heale makes good use of contemporary campaign literature, but he is hardly accurate in regarding such material as "curiously little-used by other studies of early presidential elections" (p. ix). The author's decision to separate the quest for the presidency from other aspects of politics leaves at times incomplete impressions of the American political process. He sees Jackson as a misleading example of presidential authority during the period of the second party system, thus ignoring the strong assertion of presidential authority by Polk. Although Jackson and Polk together governed for half of the twenty-four years from 1829 to 1853, Heale, in seeking long-term patterns, is prone to give more emphasis to the number of presidents and candidates crowded into the ten years after 1840, a period in which two presidents died in office and both of the vice presidents who succeeded them were not nominated for president.

In comparison with Richard P. McCormick's recent *The Presidential Game*, which focuses on the same era, Heale's *The Presidential Quest* offers fewer new insights and a less engaging presentation but may serve to supplement McCormick's more commanding work.

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University of Missouri,
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OTTO MAYR and ROBERT C. POST, editors. *Yankee Enterprise: The Rise of the American System of Manufactures*. (Symposium Sponsored by the United States Chamber of Commerce.) Washington: Smithsonian Institution Press. 1981. Pp. xx, 236. Cloth \$19.95, paper \$9.95.

Yankee Enterprise brings together nine papers by leading scholars, first presented at the Smithsonian Institution's symposium, "The Rise of the American System of Manufactures," in March 1978, together with a cogent introduction by the editors, Otto Mayr and Robert C. Post. It is a valuable book for all American historians, since its contributors underscore both how important the history of American mass production is and how inadequately it has been understood. While we still lack a full-scale interpretation, the constitutive elements of "the American system," these various essays make clear, are far more complex and unresolved than textbook accounts would suggest.

The very phrase "the American system of manufactures" was an English coinage. Following the

sensational displays of mass-produced locks, revolvers, rifles, reapers, and other mechanical products at the 1851 London Crystal Palace Exhibition, a commission of British experts rushed to the United States to study the workings of what one called "the American system." The proper meaning of that phrase remains in dispute. A. E. Musson of the University of Manchester questions to what extent mass-production engineering in the mid-nineteenth century may properly be regarded as "American" at all, since the technology originated in Europe, especially Great Britain, and at the time of the British commissioners' study, "except in woodworking, small arms, and a few other light metal manufactures, British engineering was definitely more advanced" (p. 42). Paul Uselding emphasizes the vital role played by accurate measuring devices and methods and assesses the distinctive contributions of American and European engineers. In a related essay focusing on the United States Army Ordnance Department, Merritt Roe Smith stresses the importance of governmental support in the development of precision production and, even more important, in the diffusion of systematic machine processes from armories to a host of other machine-tool industries in the second half of the nineteenth century. David Hounshell takes up a chapter of the story where Smith leaves off, as he traces the halting introduction of armory practices in the nascent sewing machine industry. Both Hounshell and Eugene Ferguson, another contributor, argue forcefully that the essential characteristics of "the American system" have too often been conflated with the specific features of interchangeability in armory production, whose makers valued and with governmental contracts could pay for high precision that in other industries would have been a luxury.

The ramifications of the development of American mass production, of course, extend far beyond specific industries and transferences. In three especially lucid and suggestive essays, Nathan Rosenberg discusses the economic and social conditions in nineteenth-century America particularly conducive to the development of mass production, Alfred D. Chandler analyzes the intimate connections between the development of American mass production and the rise of modern industrial management and marketing, and Neil Harris examines "the creation of an American system of consumption" (p. 189) and the evolving consumer consciousness in the late nineteenth and early twentieth centuries. The role of the American worker in the development of mass production is addressed by Daniel Nelson. In an interpretation certain to arouse controversy, he argues that American precision machinery manufacturers in the late nineteenth century created a distinctive kind of factory with "a near-ideal work environment, one that encouraged technical creativ-

ity, economic advancement, and social mobility" (p. 172).

Such essays hardly add up to a neat synthesis, as the editors are the first to admit, but they provide an important sampler of current scholarship and a challenge to further investigations.

JOHN F. KASSON

University of North Carolina,
Chapel Hill

HAROLD M. HYMAN and WILLIAM M. WIECEK. *Equal Justice under Law: Constitutional Development, 1835–1875*. (New American Nation series.) New York: Harper and Row. 1982. Pp xv, 571. \$20.95.

This book is a remarkable synthesis of U.S. constitutional and legal developments during westward expansion, the Civil War, and Reconstruction. Much of the recent scholarship on private law issues—contracts, torts, and corporations—and on such public law issues as the role of courts is included. There is an excellent detailed discussion of the evolution of legal philosophy from instrumentalism to formalism. As in all such books there is not much that is new for specialists, but the insight and details are as rich as anyone could hope.

Harold M. Hyman and William M. Wiecek begin by emphasizing the endurance of the constitutional priorities of popular sovereignty, popular democracy, preservation of the Union, and protection of slavery from the earliest constitutional period to the 1830s. These same priorities persisted during the period under discussion. Next they discuss the evolution of public law in the courts. Federal courts moved from using higher law principles to the practice of voiding state legislation on the basis of specific constitutional clauses, but state courts continued to use higher law principles, associating them only gradually with such specific constitutional provisions as the due process clause of state constitutions. They describe judicial protection of property rights including private corporations within the limited span of control affirmed in *Dartmouth College v. Woodward* (1819).

In nonslavery cases the authors believe the Taney court influenced the whole of the law to an important extent. The court modified the Marshall contract law decisions to permit viable state regulatory power while continuing to protect corporations. The justices used the police power to reinforce state regulatory power and the political question doctrine to enforce the notion that law ultimately derives not even from judges but from the people. The commerce power decisions were polluted by Taney's desire to protect state control of one particular kind of commerce, slavery.

Chapters 4 through 8 discuss the well-worn path

of the legal history of slavery from the colonial period through 1851 and as an issue in the Civil War. Hyman and Wiecek make a major contribution in emphasizing that Article I, section 8, and Article IV, section 4, of the Constitution protecting the states against domestic violence including slave insurrections were important elements of the pro-slavery compromises of the conventions. Although other scholars, including Staughton Lynd (1957) and myself (1971) have made this point, it seems not to have heretofore gained general acceptance in the literature.

The authors include ample treatment of the major issues during the Civil War, including civil liberties matters and the nature of the war and the Reconstruction that began even as the war proceeded. Their excellent discussion of the issue of race during the war includes a penetrating analysis of the public understanding of the nature and scope of civil rights.

They make a persuasive case, based on the work of Stanley Kutler and others, for the central role of the Supreme Court in ensuring its own independence as an institution during Reconstruction. The *Garland*, *McCardle*, and *Milligan* cases are placed in the proper context of reinforcing Supreme Court ascendancy that could be exploited on other issues in the late nineteenth century.

The authors carefully explain the misinterpretation of the plenary power of the Civil Rights Act of 1866 as reflected in court decisions before the *Slaughterhouse* cases (1873). The case of *Blyew v. U.S.* (1871) they describe as one of the most pernicious of these. The case involved a black woman murdered by whites in Kentucky. Despite the Civil Rights Act of 1866 that gave blacks the same rights as whites to give evidence, the Supreme Court upheld the state's rejection of the testimony of black witnesses because no person was being denied rights: the woman was dead and therefore no longer a person. Their discussion of this case would have benefited from a series of law review articles by Kenneth Tollett.

The book has an especially good discussion of the success of Justice Stephen Field and others in restricting the use of the Fourteenth Amendment in race cases and in mistakenly infusing a state action requirement into the Civil Rights Act of 1866. They discuss the importance of the *Hodges* case of 1906 in ratifying this retreat and rendering the Civil Rights Act an impotent instrument for black protection in the future. They note that the act received no further interpretation in the Supreme Court until 1968 when *Hodges* was overruled, but they do not note that blacks begged the Justice Department to use the act to protect them and that the Justice Department refused to bring suits, thus making it well nigh impossible to adjudicate cases and to gain further interpretations.

Wiecek and Hyman have produced an outstanding work of synthesis for both students and scholars. It has few but important weaknesses. There is almost a complete absence of discussion of legal and constitutional issues affecting Indians, Mexican-Americans, and immigrants. The treatment of women's issues is shallow. This failure may be in part due to the fact that not as much readily retrievable scholarship has been produced in these fields as in the standard areas. As one should expect, the authors deal from their own strengths, the work they have done in the past, and the fields they know best. But there is also a curious failure to use the vast law review literature on some of the issues discussed and published work in the fields of minority and women's history that includes legal matters. Despite the valuable synthesis they have produced, we still need a comprehensive constitutional history of the period under discussion.

MARY F. BERRY
Howard University

C. EDWARD SKEEN. *John Armstrong, Jr., 1758–1843: A Biography*. Syracuse, N.Y.: Syracuse University Press. 1981. Pp. xii, 277. \$22.00.

Anyone familiar with American history from the Revolution to the War of 1812 has seen the name of John Armstrong. As a young Revolutionary officer, he authored anonymously the Newburgh Addresses. While active in Pennsylvania politics during the Confederation, Armstrong wooed and won Alida Livingston; the Livingston dynasty was extremely important in New York State, and entry into the family meant wealth and position for the bridegroom. Twice between 1800 and 1804 he served in the United States Senate. In the latter part of 1804 Armstrong was tapped to succeed his brother-in-law, Robert R. Livingston, as minister to France. When the fifty-one-year-old diplomat returned home in 1810, he was praised by his countrymen for defending America's neutral rights at Napoleon's court. He was also seen as a candidate for a variety of posts, the presidency included. Finally, during the War of 1812, Armstrong served James Madison as secretary of war. Considering his career, it is astonishing that Armstrong has not had a biographer until C. Edward Skeen.

Skeen speculates that a preoccupation with "great men" explains Armstrong's neglect historically. An equally valid explanation, one that Skeen acknowledges repeatedly, is that Armstrong was a critical, disputatious man, seldom happy with himself or others. Thus Armstrong could write his brother-in-law that "the nearer you approach [Thomas] Jefferson, the better you like him" (p. 45); he read Jefferson's *Notes on Virginia*, however, and found the

Virginian's grammar and reasoning deficient. Indeed, throughout his life, Armstrong was locked in verbal combat with someone: with Barent Gardenier, for example, when Gardenier discovered Armstrong's authorship of the Newburgh Addresses; with almost everyone associated with him in Paris including James Bowdoin, James Monroe, Fulwar Skipwith, and David Bailie Warden; his actions as secretary of war from 1812 to 1814 caused enough controversy to occupy Armstrong's pen down to his death in 1843. These battles, especially with Monroe, hurt Armstrong enormously. Thus, Jefferson was describing his former minister in 1811 as "cynical & irritable & implacable."

Skeen deserves a wide audience, for he has written a straightforward, balanced, and generally well-researched biography, no small accomplishment considering his pugnacious subject. Along the way, Skeen also corrects Henry Adams and Irving Brant, and he ably defends his interpretation of the "Newburgh Conspiracy." While explaining events from Armstrong's perspective, Skeen minces no words in discussing the general's shortcomings. If Armstrong had a first-rate mind, he was also lazy. As a diplomat, he was ill suited to his Parisian post. In the cabinet as secretary of war, Armstrong, while inconsistent and vacillating, also advanced able young commanders and improved the supply system. Ultimately, Skeen finds Armstrong to be his own worst enemy, for "he never hesitated to utter the unvarnished truth, at least as he saw it, regardless of consequences" (p. 229). This biography may even now be causing some rumbling in the Rhinebeck, New York, cemetery where Armstrong lies.

CLIFFORD EGAN
University of Houston

CHAR MILLER. *Fathers and Sons: The Bingham Family and the American Mission*. (American Civilization.) Philadelphia: Temple University Press. 1982. Pp. xi, 308. \$22.50.

Char Miller has tackled a large and fascinating subject—the Bingham family of Connecticut. In this multigenerational biography he describes the lives of five Bingham who gave themselves to public service, each believing in his own way that his efforts would usher in a better world. Despite their disparate vocations and personal beliefs, these Bingham are all missionaries, driven by an urge to shape the world to the requirements of a deeply felt ideal. Theirs is a remarkable family history of nonconformity, moral insurgency, and generational conflict.

Miller begins his story in 1819 with the decision of Hiram Bingham, Sr., (1789–1869), a young Congregational minister, to dedicate himself to the foreign missionary service. That year Bingham

sailed out of Boston on the brig *Thaddeus*, the leader of the first American missionary group to the Sandwich Islands. A self-styled "ambassador to guilty men," his twin aims were to convert the islanders and to spread Yankee ways. Less than a decade after his arrival in the islands, he was preaching in a pole-and-thatch tabernacle in Honolulu to a congregation of two thousand converts. Around the Hawaiian mission he became known as "King Bingham," and for twenty years he ran the settlement there with a hard-fisted discipline.

Hiram, Sr.'s work for the gospel was carried on by his son, Hiram, Jr., (1831–1908), who, after laboring for six difficult years in the Gilbert Islands, was forced by ill health to return to Honolulu, where he devoted the remainder of his life to providing a Christian literature for the Gilbert Islanders. He, in turn, marked his son, Hiram III (1875–1956), to carry forward the family's Christianizing effort in the Pacific. But after training at the mission in Honolulu and later at Yale, Hiram III abandoned plans to enter the ministry, married an heir to the Tiffany fortune, and launched a wide-ranging career in teaching, exploration, aviation, and politics. A scholar-adventurer in the manner of Theodore Roosevelt, Bingham organized and led a series of path-breaking geographic expeditions to South America, retracing Bolivar's march to liberation and rediscovering the famed "Lost City" of Machu Picchu, the citadel the Incas had built high in the Andes to escape the Spanish. His published accounts of these travels and discoveries helped to incite interest in the United States for the serious study of Latin American history.

After serving in the United States Army Air Service in World War I, Bingham entered Connecticut politics and was elected lieutenant governor, governor, and United States senator. In the Senate in the 1920s he vigorously championed the interests of the conservative wing of the Republican party and pressed for a stronger American economic and military presence in Latin America. He "strayed from the 'path of righteousness,'" in Miller's words, "and found comfort in the secular ideal of Mission—empire."

In 1929, Hiram Bingham III was censured by his colleagues for a breach of senatorial ethics. This killed his political career. Several years later his eldest son, Alfred, founded *Common Sense* magazine, an organ of non-Marxian socialism. As a radical organizer and theoretician Bingham worked to develop a radical program and party issuing from the unique conditions of American civilization and appealing to the middle classes as well as labor. He possessed in a secular form the same commitment to moral service that drove his forebears to establish a Calvinist stronghold in the South Pacific.

After 1936 Bingham shed his radicalism and

became a New Deal Democrat. Beset by personal problems, he dropped out of active political life after 1945.

But his son Stephen continued the family tradition of generational rebellion and moral service, becoming an activist radical lawyer in the 1960s. A Marxist-Leninist, Stephen was indicted in 1971 on five counts of murder for allegedly smuggling a gun to the imprisoned Black Panther leader George Jackson who, minutes after Bingham's visit to San Quentin, started a riot that led to his own death and the death of three guards and two fellow prisoners. That day Stephen Bingham went into hiding and has not yet been found by authorities.

Miller's book is based largely on correspondence between Bingham fathers and sons. He argues that it is, among other things, a study of the "father-son relationship," of the ways Bingham sons were trained to accept and advance their fathers' moral beliefs, and of how these "parental beliefs were absorbed, challenged and reformulated." And that is what the book is, in the main. It is certainly not a convincing account of "the changing character of the American mission." Nor is it a full family biography. It is far too incomplete to fulfill that claim.

But even as a modest study of parental relations, the book is disappointing. Few families left as full a record as these Bingham. Miller has scrupulously examined this material. But he retreats from the biographer's responsibility to probe the inner life of his subject, to search for motivation, to seek to explain, not merely to describe, the generational conflicts and the commitments to the moral reform that will continue to draw historians to this most remarkable American family.

DONALD L. MILLER
Lafayette College

LAWRENCE J. FRIEDMAN. *Gregarious Saints: Self and Community in American Abolitionism, 1830-1870*. New York: Cambridge University Press. 1982. Pp. xi, 344.

Lawrence J. Friedman, in this his third book, has presented a fresh and challenging analysis of the antislavery movement written from a psychological perspective. Especially interested in intellectual history, he probes the minds and hearts of selected abolitionists, finding therein a great deal of ambivalence and ambiguity.

He writes mainly of "first-generation immediatists" who became abolitionists in the early 1830s and continued active until after the Civil War. He sees them as products of experience in the various organizations that constituted "the Benevolent Em-

pire" of the 1820s. He notes that many of them had supported the American Colonization Society but had come to the conclusion that it was fundamentally a proslavery organization and rebelled against its program. He refers to these individuals as "pious young evangelical missionaries" (p. 16).

Friedman singles out for detailed examination three abolitionist "clusters" or "intimacy circles." The first of these is "the Boston Clique" led by William Lloyd Garrison and Maria Weston Chapman, whom he labels as "insurgents." The second is the New York City group who followed Lewis Tappan; these he calls "stewards." The third is the upstate New York circle that gathered around Gerrit Smith, whom he refers to as "voluntarists." He discusses the leaders, characteristics, principles, and areas of agreement and disagreement in each of these groups. Within each cluster, he argues, there was persistent tension between "simultaneous cravings for cordial collectivity and pious individuality—for gregarity and sainthood" (p. 67).

From this discussion Friedman moves on to examine the evolution of ideals and policies within these clusters on gender relationships, race relations, and the use of violent means. He traces the movement within abolitionism toward equal participation by women and includes some interesting material on abolitionist marriages. He analyzes "the chord of prejudice" that troubled relationships between white and black abolitionists, finding a strong element of "ethnocentric missionary paternalism" in the attitudes of the whites. He devotes a chapter to the conflict among abolitionists over moral suasion and the justification of violence.

Friedman's next topic is the relationship between "immediatists" and the antislavery politicians of the 1850s and 1860s, whom he calls "radicals." His final chapter deals with the controversy among abolitionists as to when the antislavery societies should be dissolved.

The book is based on wide-ranging research in both primary and secondary sources. Although it has only a short bibliographical essay, there are fifty pages of reference notes. It is well organized and well written. The author's labeling of different categories of abolitionists, however, results in some awkward passages. For example, he refers to James Russell Lowell as "an insurgent friend of the Quincy-Phillips-Chapman aristocratic wing of the Boston Clique" (p. 151). His brand of psychohistory is moderate and generally convincing. While his attitude toward his subjects is critical, it is not unappreciative. Everything considered, this book represents a worthwhile addition to the large body of literature on the antislavery movement.

IRA V. BROWN
Pennsylvania State University

JOHN BARNWELL. *Love of Order: South Carolina's First Secession Crisis*. Chapel Hill: University of North Carolina Press. 1982. Pp. x, 258. \$25.00.

South Carolina differed from other slave states during the antebellum period in two significant regards: its extremism in defending slavery and its failure to develop a two-party system. Concerned primarily with the first, John Barnwell attempts in this competent, if narrowly conceived, study of the state's reaction to the Compromise of 1850 to explain the state's militance and "to chronicle" its secession crisis of 1850–51.

More than half the book sets the background for the crisis by describing the state's social structure, political institutions, and history from the nullification crisis to 1850. This portion is unabashedly derivative. Rarely, indeed, have I encountered a book that relies so heavily on direct quotations from secondary accounts to make its case, as though iteration of other historians' opinions was a substitute for evidence. Following previous accounts, Barnwell attributes the Carolinians' extremism to their long experience of living with a black majority. No other Southerners had so large a stake in slavery, he asserts. Moreover, he convincingly demonstrates, they were far more concerned with preserving slavery within their borders than with extending it westward. Precisely because they viewed the passage of the compromise as a first step toward abolition, the vast majority of white Carolinians considered secession necessary to protect slavery.

This consensus in turn caused the response to the compromise to develop along unique lines. Unlike Georgia, Alabama, and Mississippi, where new Union and Southern Rights parties emerged to contest for state and national offices on the grounds of accepting or resisting the compromise within the Union, South Carolinians divided into camps that appeared elsewhere only in 1860–61. A majority of immediate secessionists opposed a minority of cooperationist secessionists. They so divided, Barnwell argues, primarily because they responded differently to the political isolation from other slave states that South Carolina had endured since the nullification controversy. Convinced by that experience that they could expect no help from other states until secession was a *fait accompli*, separatists demanded that South Carolina act alone to save slavery. Cooperationists warned that separate action would only increase the state's isolation and thus doom slavery and the social order that rested upon it because only a united South could successfully defend slavery. By the time of the decisive election in October 1851, events outside the state had added weight to the cooperationists' case, and the separatists were defeated.

Barnwell's analysis of why South Carolinians divided as they did is plausible, yet he makes no attempt to explore alternative explanations by identifying the social bases of the leadership and voting support of the two groups. More disappointing, he fails to exploit the opportunity for comparative analyses with Georgia, Alabama, and Mississippi, states whose politicians and voters had long experience with two-party competition, to show how South Carolina's unique partyless condition may have shaped developments there. Barnwell, for example, argues that Carolinians divided into two "parties" over secession, yet those ephemeral organizations hardly seem like parties at all compared to Union and Southern rights parties elsewhere. They elicited no permanent allegiance from leaders or voters and quickly disappeared. More important, they did not contest for state or national office or even for seats to a state secession convention. Rather, the decisive election was for delegates to a Southern congress that had been called by the Nashville Convention and that everyone knew would never meet. Had the separatists been able to bring the matter to a clear vote earlier in some other election, Barnwell's evidence strongly suggests, they might have succeeded.

Compared to other states, in sum, the political options and outlets available to South Carolinians seem both limited and unreal. Had Barnwell adopted an explicitly comparative approach to the ways in which different political cultures and structures shape events, it may have suggested that South Carolina's unique nonpartisan politics, and not just its supposedly unique stake in slavery, contributed both to its militance and to the failure of its first secession movement.

MICHAEL F. HOLT
University of Virginia

LAWANDA COX. *Lincoln and Black Freedom: A Study in Presidential Leadership*. Columbia: University of South Carolina Press. 1981. Pp. xiii, 254.

This is a fascinating book. LaWanda Cox sets out, first, to review Abraham Lincoln's attitude toward the black people of the South during the Civil War and then to assess the possibility of any Reconstruction program's success in giving civil rights to the freedmen and maintaining those rights. To the best of this reviewer's knowledge there has never been a book quite like it, although the author does owe, and acknowledges, a debt to Peyton McCrary, whose *Abraham Lincoln and Reconstruction: The Louisiana Experiment*, went over some of the same ground.

Like other recent writers, Cox does not accept the idea of a moderate Lincoln contesting with Radical

Republicans with whom he had fundamental disagreements and pulled by them against his will into emancipation and advocacy of limited black rights. Instead she sees Lincoln as quite as much in favor of the Radical program as any of the Radicals, but much more aware that politics was "the art of the possible."

Half of the book is taken up with a review and reinterpretation of events in Louisiana between the fall of New Orleans in 1862 and Lincoln's death three years later. Delving deeply into the labyrinth of Louisiana politics, Cox concludes that it was Lincoln's definite intention that black men in Louisiana should have the right to vote. Lincoln's letter to Governor Michael Hahn in which he "barely suggested" that the Louisiana state constitution then in the making give the vote to some blacks was not really "a suggestion . . . to you alone," but rather a directive. This directive, she believes, would have been carried out had not a split developed in Louisiana between General Nathaniel P. Banks and his allies on the one hand and more Radical Unionists led by Thomas Jefferson Durant on the other. As it was, Banks was prepared to make an effort to get a black suffrage provision approved by the all-white legislature elected in 1864 when Lincoln was assassinated and Andrew Johnson's accession to the presidency brought a two-year-long reaction.

LaWanda Cox is a superb writer, and her arguments are in the main plausible when not completely convincing. It is only fair to point out that the words "may have" are used a number of times in reaching debatable conclusions. She pays almost no attention to General Banks's political ambitions as a factor in the Reconstruction of Louisiana, and she treats Durant more as an eccentric strict constructionist (who favored black suffrage!) than as the friend of liberty he has seemed to other students of the Civil War and Reconstruction. On the other hand, her conclusion in the final chapter that, no matter what Lincoln or anyone else might have done, lasting rights could not have been given to freedmen in 1864 or 1865 any more than in 1867 would be extremely difficult to challenge.

This book is "controversial" in the best sense of that word. It is required reading for students of presidential Reconstruction and recommended reading for anyone interested in Lincoln, the Civil War, Reconstruction, in general, or Louisiana.

JOE GRAY TAYLOR
McNeese State University

HANS L. TREFOUSSE. *Carl Schurz: A Biography*. Knoxville: University of Tennessee Press. 1982. Pp. viii, 386. \$29.50.

Carl Schurz has been the subject of numerous biographies, mostly in German. Now, Hans L. Trefousse, a respected American historian of German birth, has come forth with the first comprehensive biography in English of the great German-American leader in fifty years. It will be welcomed by scholars.

The first half of the book traces in careful detail Schurz's early life in Germany, his participation in the upheavals of 1848, and his resettlement in America—concluding with a detailed treatment of his career as a youthful general in the Civil War. The last half of the book examines the years from 1865 to the death of Schurz in 1906, focusing on his role as a senator from Missouri during Reconstruction, his leadership in the Liberal Republican revolt of 1872, his service as secretary of the interior under President Hayes and his subsequent identification with the Mugwumps and the anti-imperialist movement.

For all the book's merits, there are shortcomings. Schurz buffs, disappointed by the past omission of Fanny Chapman from the Schurz story, will be frustrated to find only four references to Chapman in the index and no mention of some of Schurz's other American friends. (Unmentioned, for example, is Henry Holt, who once underscored Schurz's capacity for showing love, a trait Trefousse gives relatively little attention to, while noting less-appealing sides of his subject's personality.)

Moreover, the author gives Schurz's brief military career too much emphasis. And readers who prefer dealing in spades will be put off by Trefousse's vacillation in estimating the man whom he finds, on the one hand, a self-serving egoist and, on the other hand, a worthy role model for German-Americans.

But these are minor cavils. Trefousse is correct in his estimate of his subject as a "peculiar mixture of righteousness and self interest" (p. 80). Like his fellow Mugwumps, Schurz's hard money views and his postwar disassociation from the welfare of the freedmen collided with his stated ideals. As Trefousse writes: "That America's money supply was grossly inadequate for an expanding industrial society and that deflationary pressures were placing heavy burdens on the farmers did not faze him" (p. 219). And Schurz, by abandoning the Radical cause for political reasons, showed himself "oblivious to the fact that he was in effect betraying his life long advocacy of human equality" (p. 196).

The book tempers other long-held illusions about Schurz. The author, agreeing with another writer that Schurz's mission was to be "the main intermediary between German and American Culture" (p. 295), nonetheless strives to allay the notion of contemporaries that Schurz delivered the German-American vote. This notion, sedulously cultivated by

Schurz, led presidents from Lincoln to Theodore Roosevelt to forgive him his oftentimes arrogant behavior on the assumption that "flattery of the Germans was [politically] essential" (p. 85).

Its limitations aside, this attractively published book provides scholars a judicious, well-researched, and—within the limitations noted—appealing portrait of the greatest American of foreign birth produced during the nineteenth and twentieth centuries.

WILLIAM M. ARMSTRONG
Potsdam, New York

GRADY MCWHINEY and PERRY D. JAMIESON. *Attack and Die: Civil War Military Tactics and the Southern Heritage*. University: University of Alabama Press. 1982. Pp. xv, 209. \$17.95.

Grady McWhiney and Perry D. Jamieson indicate that there are almost as many explanations for Confederate defeat in the Civil War as there are historians writing about the war. They then proceed to add another explanation, which in turn is supported by still a further explanation. Together, these interlocking explanations form an intriguing exercise in Civil War and Southern ethnic history.

Briefly, the book argues that Confederate officers and soldiers were excessively prone to offensive tactics and that the officers, including those of the highest rank, failed to appreciate the significance of the greatly increased range and killing power of the war's basic weapon, the rifled musket. Put another way, the Confederates used outmoded, eighteenth-century tactics against nineteenth-century technology. They bled themselves to defeat as an army and a nation, as well as to death individually in almost one hundred thousand instances.

The authors support this thesis with impressive casualty statistics and a penetrating analysis of Civil War tactics. They demonstrate that in the first twelve major campaigns of the war (those taking place through the autumn of 1863) the Confederate armies suffered almost forty thousand more casualties than their opponents, and that because of the relative smallness of the Confederate forces they incurred proportionately almost twice as many casualties (24.6 percent Confederate to only 13.9 percent Union). The authors contend persuasively that the rifled musket, "effective at 1000 yards," rendered obsolete many of the tactical operations that had been successful in the recent Mexican War, which was fought largely with smoothbores; that cavalry charges and the employment of field artillery on line with the infantry were now prohibitively costly in lives; and that, more important, so were infantry attacks in close formation. Yet, strangely,

many officers in both armies failed to understand the impact of the advent of the new weapon. The most influential Confederate who failed to understand it, the authors believe, was General Robert E. Lee, who during the early period of his command launched repeated attacks and almost always took greater losses than his adversaries.

From these statistics McWhiney and Jamieson conclude that the Confederacy ought to have adopted a strategy of defensive tactics altogether. They say, "by attacking instead of defending, the Confederates had murdered themselves" (p. 24). Possibly so. Perhaps some such strategy as that practiced by George Washington in the American Revolution would have been more appropriate to Confederate requirements—a strategy of avoiding pitched battles in the field and wearing down the enemy's will by hit-and-run activities and attrition. But this presupposes that the population of the Confederacy possessed the patience to endure such a strategy; and, as this book makes clear, patience was decidedly not a Southern trait.

Also, a different conclusion from that of the authors may be drawn from the statistics. The highest casualties, both absolutely and proportionately, taken by the Confederates in a single battle occurred at Vicksburg, where they employed defensive tactics. A strategy entirely dependent on defense might well have resulted in a series of Vicksburgs ending in the loss of the war. Or, consider Lee's statistics. When in the spring of 1864 he was eventually forced to resort to defensive tactics, which he is credited with conducting brilliantly, he continued to suffer proportionately more casualties than did Grant, his opponent, thus making Confederate defeat inevitable if the Northern people were willing to pay the "butcher's bill."

Why did Southerners prefer to attack instead of defend? The authors' answer to this question provides the most stimulating but controversial portion of the book. In part, they say, Confederate generals attacked because they had been taught at West Point to do so and were too "fossilized" to break with their training. But there was a deeper and more pervasive cause. The Celtic makeup of the Southern population (Scotch-Irish, Scottish, Irish, Welsh, and Cornish) produced Confederate soldiers who were extremely courageous but equally impetuous, just as their Celtic forebears had been throughout recorded history. The charge was the Celtic way of war. The "Celtic Thesis" did not originate with this study. McWhiney and his Alabama associates, Forrest McDonald and his wife Ellen Shapiro McDonald, have for some time been using it to explain most forms of Southern distinctiveness. They have mounted a bold charge into ethnic history, with a fascinating hypothesis that is probably beyond absolute confir-

mation or refutation. Those readers who reject it will find the tactical thesis of this book still not seriously impaired, though they will be obliged to supply their own explanations for the Confederate behavior.

However controversial, this is an excellent study. It embodies a high knowledge of Civil War leadership and tactics; it makes its points cogently and clearly. The casual reader will be captivated by it, and the Civil War scholar cannot afford to ignore its implications.

CHARLES P. ROLAND
University of Kentucky

CLARENCE E. WALKER. *A Rock in a Weary Land: The African Methodist Episcopal Church during the Civil War and Reconstruction*. Baton Rouge: Louisiana State University Press. 1982. Pp. 157.

A Rock in a Weary Land, Clarence E. Walker's study of the African Methodist Episcopal church during the Civil War and Reconstruction, would have been better titled "No Rock in a Weary Land." The picture Walker paints is one of a church divorced from reality, self-centered, and fanatic. Hostile to Northern white churches and to other black denominations (especially to other black Methodist groups), ashamed of former slaves' "primitive" religion, convinced of their own superiority, and determined to control Southern black religious-cultural developments, these Northern black Methodists were willing to work with Southern white Methodists after 1866 and only turned against them when these Southern whites chose to foster a Southern black "independent" church. Given this stance, many Southern blacks did not find the AME church a rock; Walker, however, never analyzes the number that joined this or other churches.

Walker's brief essay deals primarily with the AME church and its relationship to other Methodist churches. While he does analyze the black Methodists' political outreach in detail, he does not deal with their relationships to other denominations, nor, in any depth, with the former slave reaction to them. This limited purview is no doubt due to the fact that the book is based on Walker's dissertation, completed in 1976. Since that date, and prior to the publication of this book, Leon F. Litwack's *Been In The Storm So Long* (1979) has placed the black Methodist approach in the wider context of all black and white reactions to the former slaves. In one chapter Litwack succeeds in presenting both the wider institutional picture and the personal story of black and white interactions. A black women's defense of "slave" religion tells an important part of the story missing in Walker's book: "Dey tells us we mustn't make no noise ter praise de Lord. I don't

want no sich 'ligion as dat ar. I want ter go ter Heaben in de good ole way" (p. 462).

While overshadowed, Walker's book is valuable as a chronicle of the troubled hegira of the AME church: from lack of support for the white people's war to relatively full support of a war for emancipation. Moreover, Walker is very sensitive to the nature of AME church values and argues persuasively that African Methodists wanted the Southern black to become decorous, educated, motivated to self-help and middle-class economic behavior. (Walker is not willing to call these "white" middle-class values; he does, however, call their advocates "Afro-Saxons.") While the AME church wanted a "proper" and deserving black population, Walker emphasizes and documents the fact that they did not advocate waiting for white approval: they did demand immediate equality and sought to achieve it through direct political action. Walker analyzes the activities of the twenty-three AME church missionaries who were briefly successful as Southern politicians and is most concerned to judge their actions, claiming that they were not failures in that they continued to call for full equality and would not compromise. Walker's book makes a good case for their contribution while it is very critical of their values. Overall, it adds substance to a period we are just beginning to know well.

MECHAL SOBEL
University of Haifa

J. MORGAN KOUSSER and JAMES M. MCPHERSON, editors. *Region, Race, and Reconstruction: Essays in Honor of C. Vann Woodward*. New York: Oxford University Press. 1982. Pp. xxxvii, 463. \$25.00.

Seventeen of C. Vann Woodward's former students have collaborated in providing this substantial volume in his honor. The result is excitingly varied in topics and styles of presentation, sparkling with valuable insights, useful new research, and suggestive interpretations. The editors, J. Morgan Kousser and James M. McPherson, undertake the introductory assessment of Woodward's work and influence in a thoroughgoing and perceptive essay. I am sure that they will not be offended by a prediction that their's will be far from the last such assessment, though it must be the proper starting point for all to come.

Five essays appear under each of the three broad themes, region, race, and reconstruction, but the breadth and richness of many contributions defy such classification. Daniel T. Rogers explores the significance of the early demise of Howard W. Odum's regionalism concepts, which seemed to tower over Southern intellectual life in the 1930s. Bertram Wyatt-Brown addresses the changing per-

ception of slavery as reflected in the trend of proslavery arguments toward state racial regulation as a model for modernizing the institution. Steven Hahn delves into grass-roots state and local sources to provide a convincing demonstration that the class struggle in upcountry Georgia over fencing laws furnished roots for Southern Populism. Robert Dean Pope explores the difficulties he sees in the biographies of Southern politicians in the age of segregation and suggests that "We cut ourselves off from any real understanding of racism in the American South if we deny that racial views offensive to us could be—and can be—held by intelligent and decent people" (p. 108). Willie Lee Rose closes the section on region with a penetrating analysis of four episodes in popular culture: *Uncle Tom's Cabin*, *The Clansman* (and the motion picture *Birth of a Nation*), *Gone With the Wind*, and *Roots*. She concludes that no other episodes can compare with these and that each appeared when a new synthesis was forming concerning the American Civil War era and race in America.

Barbara J. Fields argues that race as a concept is "neither the reflex of primordial attitudes nor a tragically recurring central theme" but an "ideological medium through which Americans" evaded the fact that "enslavement of Africans and their descendants constituted a massive exception to the rules of sovereignty and power that were increasingly taken for granted" (p. 168). The mistaken concept of race is only the result of "acts and decisions of men and women in a society now past," she concludes "and a responsibility which, because the outcome remains provisional, we are obliged to share with them" (p. 169). Tilden G. Edelstein fascinatingly highlights the changing perceptions of miscegenation in America by tracing the theatrical handling of *Othello*. He points out that "changing American racial, sexual, and class attitudes necessitated the periodical alteration of characterization, costuming, makeup, and even dialogue" (p. 180). Charles B. Dew recreates the life of Sam Williams's family in slavery and freedom so eloquently that only the initiated could guess what skill, labor, and imagination were required to paint such a portrait from bare and sometimes fragmented surviving records. Human dignity is restored to a slave family by bringing the reader to share their life experiences. Robert F. Engs throws startling light on racial perceptions by recounting the Hampton Institute policies for educating Indians along with black students. Louis R. Harlan adds an enlightening dimension to ethnocultural perceptual thickets with his account of "Booker T. Washington's Discovery of Jews" (p. 267).

Thomas C. Holt's essay, opening the Reconstruction section, analyzes the triumph of explicitly racist ideology in the late nineteenth century in terms of

experience with emancipation in the British West Indies and the American South. He argues that expectations about the role of labor in a maturing industrial capitalism may have provoked the espousing of racism when freed black people did not conform to white capitalist preconceptions. Lawrence N. Powell makes a convincing case that damaging factionalism and patronage struggles within state Republican organizations in the South during Reconstruction may best be understood in terms of the crucial part that income from political offices played in the economic survival of individual carpetbaggers. J. Mills Thornton III provides basic data, lucidly presented in tables and figures, to suggest that fiscal policy played a perfectly understandable role in the failure of Radical Reconstruction in the Lower South. The root of the problem was the ending of a major source of state tax income with the end of slavery and the shifting of the burden from slaveowners to all landholders. William S. McFeely uses biography to illuminate the Georgia Reconstruction landscape with his portrayal of a Georgia lawyer, Amos T. Akerman, who struggled for racial justice and fought the Ku Klux Klan with some success, briefly holding the cabinet position of United States attorney general and afterward safely continuing his legal career in Georgia. Vincent P. DeSantis recounts extensively the literature and evidence on Rutherford B. Hayes's decision to remove troops from the South and provides a full historiographical study of the end of Reconstruction. The bibliography of Woodward's published writings is furnished by Louis P. Masur.

A well-recognized risk in bringing disparate essays together is the likelihood that many will be lost to potentially interested readers who do not guess their location. That risk is minimized in a collection in honor of Woodward, for anyone informed about Southern history will assume, and correctly so, that precious nuggets are plentiful therein.

THOMAS B. ALEXANDER
University of Missouri,
Columbia

BRUCE A. ROSENBERG. *The Code of the West*. Bloomington: Indiana University Press. 1982. Pp. 213. \$15.00.

No gunslinging Hammurabi in a B-feature Western ever enunciated (laconically, drawlingly, or otherwise) the "code" with which this volume deals. Indeed, Bruce A. Rosenberg employs the word in its other meaning, having (or so he believes) discerned in the bibliography of the American West a persistent set of symbols to be deciphered by astute cultural cryptographers. Predictably, because he is a

professor of literature and a folklorist, Rosenberg's recommended manuals are those by Stith Thompson, R. W. B. Lewis, Henry Nash Smith, and like-minded scholars.

The problem with Rosenberg's title, and hence with his method, is clear: to be deciphered, a thing must first have been enciphered. Those who believe that nineteenth-century diarists, journalists, and dime novel hacks laced their work with conscious and consistent cultural symbols to keep posterity guessing are likely to find Rosenberg persuasive. They will accept also the notion that Stith Thompson and others succeeded in creating a reliable periodic table of cultural elements, so that, as Rosenberg suggests, George Armstrong Custer becomes a martyred warrior with the same (culturally speaking) molecular structure and atomic weight as Charlemagne's Roland, Arthur's Sir Gawain, or Saul struggling finally against the Philistines (pp. 101–104). There are bound to be skeptics.

Rosenberg's introduction is a melange of apology and cross-purpose, much of it silly enough to have alerted the sleepest of university press editors, or so one would have hoped. Rosenberg notes, for example, that despite the Walt Disney revival of popular interest in Davy Crockett in the 1950s, "Crockett maintained his position in the elitist view of America, appearing in Parrington's *Main Currents of American Thought*" (p. 5). Did Crockett's ghost rise up to prevent the rewriting of poor dead Parrington's book or to guard the library shelves? Was Disney a participant in some metaphysical joust? The statement is astonishing in its disregard for history, but it is only one of many careless and gratuitous observations.

Rosenberg admits (p. 13) that his book "offers no major new understanding of that vast and nebulous [*sic!*] region we call the trans-Mississippi West, either historically or folklorically," but his honesty does not disarm. All scholars ruminate to set their thoughts in order (or they should), but why publish the musings when they are so narrow and so banal? Rosenberg has read little enough of history—there is no Unrau on *his* Overland Trail, no Arrington among *his* Mormons—and certainly not enough to warn him against applying principles of literary criticism to the holographic effluvia of predominantly economic phenomena. Myth is a latter-day product: miners, trappers, railroad men, Pony Express riders, and even outlaws—all Rosenberg subjects—pursued the business of getting through the day too vigorously to create the stuff on the spot. If the written record is to be analyzed, even in the manner that Rosenberg has chosen, its context may not be so blithely ignored.

WILLIAM W. SAVAGE, JR.
University of Oklahoma

MICHAEL D. GREEN. *The Politics of Indian Removal: Creek Government and Society in Crisis*. Lincoln: University of Nebraska Press. 1982. Pp. xiii, 237. \$18.95.

The Creeks were the most numerous and powerful of the Southern "civilized" tribes, and they are the least understood. One therefore welcomes this new study that joins other excellent recently published works concerning these Indians.

Michael D. Green concentrates on the 1814–36 period, the years after the defeat at Horseshoe Bend and preceding the removal to Oklahoma, and politics are his main concern. He deals with four centers of political power: the United States government in Washington; the state governments in Georgia and Alabama; the Creek central government—the National Council—that, depending on circumstances, met either among the Upper or Lower Creeks; and the local Indian government in the towns or *talwas*. The author deftly relates to each other the political maneuverings and decisions made at these four centers.

Another strength of this book is the discussion of how the Creek agency became increasingly politicized and the effects on the Indians. A good example of this was when William H. Crawford, powerful political boss in Georgia, appointed former governor David B. Mitchell to head the agency. Paying little regard to Indian welfare, Mitchell feathered his own nest and boosted the image of his mentor. The wealthy, unscrupulous mestizo, William McIntosh, Mitchell's partner, profited handsomely from trading with the Creeks, from capturing Negroes in Florida and illegally selling them in Georgia, and from accepting a *douceur* for ceding Creek lands. It was the sale of Creek territory at the 1825 Treaty of Indian Springs that cost McIntosh his life. His assassination by Creeks opposing removal has been told many times, but never better than by Green.

The author is particularly interested in the development of the National Council. He contends that as a center of traditional values and opposition to removal after 1815 it became a truly national government. Surviving writings of white Indian agents and acculturated mestizos such as Alexander McGillivray and William McIntosh support this thesis. Even so, one must wonder if fundamental political power still did not remain where it customarily had been—with the *talwa* and clan, and keeping this in mind may be the best way to understand the Creeks both in the East and even subsequently in Oklahoma. Related to clans, *talwas*, and factionalism are the Seminoles—those Creeks who during the course of a century or more had moved to Florida. From the Indian perspective, including that

of McGillivray, McIntosh, and at times the United States, the Seminoles were still Creeks. There were many like Osceola who at one point were regarded by whites as Creeks and at another as Seminoles, or possibly vice versa. To fully understand Creek politics one cannot disregard the Seminoles.

Because of the nature of the sources—or lack of them—it is inevitable that scholars will debate some of Green's conclusions. Nevertheless, based on thorough documentary research, this book is a first-rate study, revealing not only much about Indians but also about whites and unsavory aspects of Jacksonian democracy.

J. LEITCH WRIGHT, JR.
Florida State University

CLIFFORD E. TRAFZER. *The Kit Carson Campaign: The Last Great Navajo War*. Norman: University of Oklahoma Press. 1982. Pp. xviii, 277. \$14.95.

For many American Indians, that something happened is more important than when it happened. For the Navajos, the figure of Kit Carson or Bi'ée' Lichif'ii (Red Clothes) remains an immediate one. Carson played an important role in the campaign to force Navajo surrender in the 1860s. Following the orders of General James H. Carleton, Carson and his men marched into Navajo country in 1863–64 and helped prompt the capitulation of many of the Diné to temporary exile at Fort Sumner in eastern New Mexico. Though Carson was a reluctant emissary and his success militarily has been overemphasized, he is still today a primary villain in Navajo eyes.

In *The Kit Carson Campaign: The Last Great Navajo War*, Clifford E. Trafzer provides a narrative of those days. The book includes some introductory material on "The Navajos Before Kit" and a brief concluding chapter, but the heart of the study deals with the efforts of Carson and his cohorts to encourage the Navajos to end armed resistance to the new power in the Southwest, the United States. It is a dramatic story, and for the beginning student of Navajo history Trafzer's account must be compelling. In addition to such personalities as Carson, Carleton, and the Navajo leader Manuelito, the trek into the area of Canyon de Chelly and the tragedy of the Long Walk to Fort Sumner make for a tale of epic dimensions. Given his study at Northern Arizona University and a year of teaching at Navajo Community College, Trafzer has some feel for the land and the people. The book is augmented by sixty-eight illustrations and three maps, the latter very nicely drawn by Don Bufkin.

Nonetheless, one must add that it is a very familiar story. More than a decade ago, in his

introduction to *Navajo Roundup: Selected Correspondence of Kit Carson's Expedition Against the Navajo, 1863–1865*, Lawrence Kelly noted that the campaign had "been the subject of voluminous literature." Kelly's documentary collection of previously unpublished letters from the expedition did much to correct mistaken images of Carson and his efforts. *Navajo Roundup* was followed three years later in 1973 by Navajo oral historical accounts edited by a Navajo, Ruth Roessel, in *Navajo Stories of the Long Walk Period*.

These two volumes, along with the work of Frank Reeve, Frank McNitt, and Gerald Thompson, encompass the period, the events, and the characters of *The Kit Carson Campaign*. More startlingly, the correspondence on which Trafzer draws so heavily in the main body of his study is precisely that published by Kelly. While Trafzer renders some acknowledgment to the pathbreaking work done by his predecessor in the archives, most readers may not realize the immense debt owed; the citations, moreover, will not prove fully educational in that regard. This book may be the product of some years, but it remains one that in its present form did not need to be published.

PETER IVERSON
University of Wyoming

ROBERT H. RUBY and JOHN A. BROWN. *Indians of the Pacific Northwest: A History*. Foreword by ALVIN M. JOSEPHY, JR. (Civilization of the American Indian Series, number 158.) Norman: University of Oklahoma Press. 1981. Pp. ix, 294. \$24.95.

MARGARET COEL. *Chief Left Hand: Southern Arapaho*. (Civilization of the American Indian Series, number 159.) Norman: University of Oklahoma Press. 1981. Pp. xiv, 338. \$15.95.

The hybrid discipline of ethnohistory has produced searching critiques of older models in the writing of Indian history, particularly the use of the tribe as an organizing device. Tribes seem an artificial, albeit convenient, substitute for the complex, fluid reality of native political, social, and cultural relations. John Collier's Indian New Deal, for example, has been criticized for focusing on tribal revitalization at the expense of local community organization, thereby promoting a bureaucratically expedient centralism foreign to the Indians' traditional "localist orientation" (band, village, faction). In the same period that Collier was trying to launch a revolution in Indian affairs based on a possibly erroneous tribal premise, the University of Oklahoma Press launched the single most ambitious publishing program in the field of Indian history, the "Civilization of the American Indian Series." While the series, now

almost half a century old, has been uneven, several outstanding works distinguish it. Tribal histories have been its mainstay, along with case studies in Indian-white relations and, as the two volumes under review suggest, regional histories and biographies.

Robert H. Ruby and John A. Brown, authors of histories of the Spokane, the Cayuse, and the Chinook, and a biography of the Sinkiuse leader Moses in the same series, have attempted something different in *Indians of the Pacific Northwest*, a synthesis drawing on their detailed knowledge while avoiding some of the pitfalls of the tribal history. The time span covered is conventional, from earliest white contact to the twentieth century, encompassing the period of dramatic change as the impact of white culture on native cultures produced unvarying results: the suppression of native ways by the missionary, the teacher, and the soldier. Setting out to view events "from the standpoint of our subjects" (p. ix), Ruby and Brown have familiarized themselves with the pertinent anthropological literature. But they have not been able to surmount the difficulties inherent in writing Indian history from white sources, and theirs remains a fairly standard history of Indian-white relations. The authors have been at pains to identify individuals and groups and to pinpoint geographical references; they know the territory, and their text is grounded in specifics. The many maps are clear and helpful, and the authors occasionally stand back to survey the scene or turn a pithy phrase. But on the whole their prose is so clogged with information that it defeats another of their expressed objectives, "to appeal to the general reading public" (p. ix). The early chapters in particular are dense and slow-moving—a problem compounded by the transposition of pages in chapter 1: once the authors get to more familiar ground the narrative pace picks up, notably in their discussions of the Modoc and Nez Percé wars. The abundant illustrations do enliven the text, but I expect that *Indians of the Pacific Northwest* will be more consulted than read, serving as a handy, one-volume reference work.

To turn from *Indians of the Pacific Northwest* to *Chief Left Hand* is to turn from too much to too little. The illustrations in Margaret Coel's biography of the Southern Arapaho leader born in the early 1820s and killed at Sand Creek in 1864 reveal an odd omission: there is no likeness of Left Hand. This omission proves prophetic, however, for despite research in manuscript collections, government documents, and secondary sources and an obvious sincerity of purpose, Coel has been unable to rescue Left Hand from obscurity. Indeed, she has not been able to locate enough solid information on this English-speaking peace chief to fill a short biographical sketch, let alone a full-length biography.

Her prose is peppered with "possibles" and "probables" and "almost certainlies" as she speculates on what Left Hand might have been doing at any given moment. But even with her conjectures—on such basic matters as when he was born, when he married, whom he married, how many children he had, what sex they were, whether he took other wives, and when he became a chief—Coel's material bearing directly on Left Hand is so skimpy that her book cannot pass as a biography. Rather, it is a history of the events leading up to Sand Creek in which the Arapaho Little Raven, the Cheyenne Black Kettle, Colorado Governor John Evans, and the controversial "fighting parson" John M. Chivington all figure more prominently than Left Hand. *Chief Left Hand* offers a generally reliable and readable narrative with a pronounced pro-Indian viewpoint. But it does not add significantly in tone or substance to Stan Hoig's *The Sand Creek Massacre* published by the University of Oklahoma Press over twenty years ago.

BRIAN W. DIPPIE
University of Victoria

LORETTA FOWLER. *Arapahoe Politics, 1851–1978: Symbols in Crises of Authority*. Foreword by FRED EGGAN. Lincoln: University of Nebraska Press. 1982. Pp. xx, 373. \$26.50.

Unlike other Plains tribes, the northern Arapahoes have maintained remarkable political stability. Until 1934, United States Indian policy attempted to destroy tribalism and reduce the influence of chiefs who attempted to foster traditional Indian ways of life. By mid-nineteenth century the Indian office began intervention into tribal political institutions by designating leaders to implement Indian policy and to be accountable for fulfillment of treaties negotiated with the United States government. Conflicts between traditional chiefs and the Indian service personnel extended for decades, intensifying as Indians were restricted to reservations. Pressure to modify tribal political institutions was further increased after the 1887 Dawes Act, which it was hoped would speed Indian acculturation and assimilation into American society.

Loretta Fowler sustains the thesis of *Arapahoe Politics, 1851–1978* most convincingly. She maintains that: "The differences between generations . . . have worked to facilitate rather than hinder adaptation to new political realities. Native religion remains a focal point of Arapahoe life and plays a key role in the legitimization of secular authority and the validation of political innovation" (p. 2). The northern Arapahoes' political structure did not crumble under the impact of federal Indian policy or the pressures of the dominant white society. Rather,

northern Arapahoes maintained their political and social ideals and survived political crises by perpetuating continuity based upon their traditional customs and mores.

How did the northern Arapahoes maintain control of their political affairs? The Arapahoes were among a few Plains tribes that possessed a social organization in which males of the same age passed through a series of ceremonial age-grade societies. Each age-grade society complemented the functions and responsibilities of other societies so that Arapahoe life was orderly and structured. The seventh age-grade society contained the elders and priests, males sixty years old or more, who generated tribal consensus on all issues affecting tribal welfare. The elders designated chiefs who were expected to follow tribal consensus in their relations with other tribes or the United States government. Those Arapahoes who aspired to chieftainship or election to the Arapahoe business council, initiated in 1908, needed the approval of the elders before they could win elective offices. When the United States government attempted to undermine the elders' power by elevating younger, educated, and more progressive men as tribal spokesmen, particularly as tribal business council members, the elders retained their influence by giving approval to individuals who could work with federal officials and still represent the tribal consensus. Respect and deference for the elders' wisdom and sacred authority allowed the northern Arapahoes to retain control of their own people, yet adopt new political forms required by white society.

Arapahoe Politics has many merits. Although Fowler's study principally focuses on northern Arapahoe politics, supernaturalism, ritualism, and economy, other aspects of Arapahoe life are incorporated into the chronological narrative. Fowler ranges widely for sources, supplementing field work with archival and manuscript records, federal documents, newspapers, and secondary sources. Rarely do scholars combine anthropological and historical methodologies as successfully as Fowler in this important volume. Errors in *Arapahoe Politics* are few and minor. But Fowler profitably could have used the studies of Francis Paul Prucha, William T. Hagan, Arrell M. Gibson, and Kenneth R. Philp to provide a clearer context of federal Indian policy for northern Arapahoe politics. Nevertheless, *Arapahoe Politics* is a key study that may lead scholars to re-examine other instances of tribal political continuity and adaptation.

DONALD J. BERTHRONG
Purdue University

LEE SOLTOW and EDWARD STEVENS. *The Rise of Literacy and the Common School in the United States: A Socioeco-*

nomic Analysis to 1870. Chicago: University of Chicago Press. 1981. Pp. xii, 247. \$20.00.

No one will ever know exactly how many people were illiterate in early America. It is clear, however, that literacy rose dramatically during the first several decades of the nineteenth century. Indeed, at the start of the century 42 percent of U.S. Army enlistees were illiterate. By 1850–59 the proportion had been cut nearly in half to 25 percent, and in the last decade of the century it was only 7 percent (p. 52). If the census is to be believed, army enlistees were more illiterate than the general population, and, of course, major differences in literacy rates between regions, ethnic groups, and classes persisted. Nonetheless, within every group, illiteracy plummeted.

How are we to account for this decline in illiteracy? Did its pace differ between classes, ethnic groups, or regions? What was its significance? These critical questions for historians are among the most important issues addressed by Lee Soltow and Edward Stevens who are the first scholars to examine literacy in America at a national level over a long period of time. By adding the results of their own extensive research to the work of other scholars (especially in the United States, Canada, England, and Sweden), they have made a notable contribution to the small but growing body of scholarship on the history of literacy.

Soltow and Stevens present data on trends in literacy based on aggregate regional and county-level data, and they analyze the individual correlates of literacy and school attendance through national samples drawn from the manuscript censuses of 1860 and 1870 and other sources, of which the most interesting is the records of army enlistees from 1799 to 1894. They complement their quantitative analysis with extensive discussions of the ideology of literacy and schooling, the spread of common school education, and the development of nonschool sources of literacy, especially printing and newspapers. As well, they preface their substantive work with a thoughtful and discriminating analysis of the various meanings of literacy.

Soltow and Stevens stress the role of population density in promoting literacy. Everywhere they find a high correlation between density and literacy, which, they argue, occurs because population concentration makes possible the accumulation of a tax base and other resources, especially schools, necessary for the reduction of illiteracy. Indeed, they argue strongly for the role of common schools in reducing illiteracy. To them, the promotion of literacy through schooling was deliberate social policy, a form of "cultural intervention" into family life designed to ensure social order and cohesion and to foster economic mobility. Judging by the results of Soltow and Stevens's research, the campaign to

promote literacy through public schooling succeeded because in all of their ecological analyses the extent of schooling (measured in various ways) was the factor most associated with a decline in illiteracy over time. Without schools, Soltow and Stevens argue, demographic processes probably would have led to a decrease in illiteracy, but the pace would have been much slower, far too leisurely to suit the promoters of the time.

Their discussion of the individual correlates of literacy and school attendance reinforces the growing body of work in these areas by pointing out the complex roles of wealth, sex, and ethnicity. Especially interesting are two of their conclusions: first, birth order influenced school attendance. Older children were less likely to go to school than their younger brothers and sisters. This general pattern (which highlights the continuation of a family economy partly dependent on the earnings of children) appeared especially vividly among families headed by illiterates, whose poverty probably made the employment of their older children essential. Second is the intergenerational character of illiteracy. Children of illiterates were themselves much more likely to be illiterate than were children whose parents could read and write. Thus, in at least one respect nineteenth-century reformers were right. There was a minority of families for whom only schooling offered a quick way to break the cycle of illiteracy and its usual (though not constant) companion: poverty.

One other important trend that Soltow and Stevens point out (but do not explain very well) is the decline in school attendance between 1860 and 1870. Other scholars have found a similar pattern. School attendance did not increase in an unbroken, linear pattern. Rather, it fluctuated with economic conditions and changes in the youth labor market. Indeed, more attention to the youth labor market would have strengthened Soltow and Stevens's book. So would a fuller exploration of other aspects of their data, especially distinctions among occupational groups. Most of their tables distinguish only between farmers and nonfarmers and do not show the important differences in schooling and literacy within the nonfarm groups. Also, they base their analysis of school attendance on families with both parents present, even though throughout the nineteenth century women headed a significant fraction of families with children. (My own research shows that the absence of a father at home often had a significant effect on school attendance.) Incidentally, either the authors or their editors should have modified the unfortunate phrase—"nuclear children"—with which they describe their sample.

Another difficulty is the theoretical thinness of their explanations. Too often they fall back on a

loose and undefined use of "modernization" and "differentiation" without paying any attention to the extensive body of criticism that these concepts have received in recent years. Nor is their reliance on population density ultimately satisfying. Populations do not concentrate automatically; there are important differences between types of urban areas; and, without a commitment to its importance, population density by itself will not promote mass literacy. As far as I can tell, capitalism, as a term, appears only once in this book; in its place the authors substitute "Victorianism," which, at best, is a loosely descriptive label that explains nothing. In far too many books, modernization and Victorianism too easily become comfortable euphemisms with which to slip by the tough questions about the dynamics of social, economic, and cultural development in the American past. One of those questions is the connection between literacy, schooling, and the nature of American capitalism.

Of course, Soltow and Stevens do not pretend to have answered all of the questions about literacy and schooling in nineteenth-century America. Indeed, their intelligent, thoughtful book opens as many issues as it resolves. Because they are, if anything, excessively modest, I want to stress that this is an important book for students of American social and cultural history.

MICHAEL B. KATZ
University of Pennsylvania

ANNEMARIE BUTTLAR. *Rassisch getrennte Schulen im Süden der USA, 1890–1950: Politische, rechtliche und ökonomische Faktoren*. (Marburger Beiträge zur Vergleichenden Erziehungswissenschaft und Bildungsforschung, number 15.) Munich: Minerva. 1981. Pp. xi, 305. DM 30.

This is a study of racially segregated schools in Kentucky, Alabama, Tennessee, and Mississippi between the 1890s and the 1950s by Annemarie Buttler, a German scholar, who studied with John Hope Franklin. Thoroughly grounded in quantitative and qualitative sources, its governing assumption is that educational, social, and economic changes went forward hand-in-hand. The author also discusses court cases, laws, strategies, tactics, and the ideologies of both races and of the various groups within them.

Much of the discussion is necessarily familiar: fewer blacks than whites went to school; whites tended to go longer as did the children of the well-to-do; white schools got more than a fair share of school funds. One's life chances varied directly with race, status, and wealth.

Other parts of the discussion are provocative and the organization of the work is highly imaginative. The book begins with a consideration of the Grady-Cable debates; the author contends that both thought black people inferior. Their difference: Grady based human rights on class and race; Cable thought rights should be the reward of virtuous conduct. Following a brief consideration of Booker T. Washington, the author's attention turns to a precedent for *Plessy v. Ferguson*: *Roberts v. Boston*, involving a suit by a black Boston minister seeking admission for his daughter to a white school. In this case the lawyers (including Charles Sumner for the plaintiff) and the judge, Lemuel Shaw, rehearsed just about every argument to be used in the next one hundred years. There follows a discussion of black legal strategies that in their relentless demand for precise definitions of "equal" and "colored" anticipated the strategies successfully pursued by the NAACP in the cases that preceded *Brown v. Board of Education of Topeka*. Indeed one of the author's great virtues is her awareness that black people were not simply responding to white pressures; they were trying to develop autonomous communities and responsible and effective political programs. This in turn became possible as a result of the Civil War and Reconstruction.

The best illustration of the point occurs on page 129. An old black farmer recalls that in the days before the teacher came he had lived in a log cabin, rented his land, and had to mortgage his crop for food. Then he was called Ol' Jim Hill. After the teacher, he got out of debt, acquired a painted house, put money in the bank, educated his children, and paid taxes. People now addressed him as Mr. James Hill. Indeed, freedom was better than slavery.

Buttlar concludes that what the South really needed was a Marshall Plan, white leaders who recognized the wastefulness of a dual economy, black leaders who saw the need to go beyond the Tuskegee concept that could never break through the fatal circle of poverty, inadequate education, and poor life chances. A direct assault on segregation was most important. Black leaders began the assault during World War I, and by 1954, the author believes, segregation was no longer acceptable to black leaders and as a legal concept was dead even before the Brown decision.

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KATHLEEN D. MCCARTHY. *Noblesse Oblige: Charity and Cultural Philanthropy in Chicago, 1849-1929*. Chicago:

University of Chicago Press. 1982. Pp. xiii, 230. \$20.00.

The revival, extension, and redefinition of social history, an enterprise launched some fifteen years or so ago by scholars working with new materials and informed by new perspectives and methods, has in recent years led to the publication of a number of elegant works on subjects long neglected. Count Kathleen D. McCarthy's account of diverse philanthropic and charitable efforts in Chicago during the last half of the nineteenth century and the first third of the twentieth as among the best. Sensitive to complex forces of social theory, social structure, community dynamics, and gender roles, she provides important and original insights into the attitudes and functional modes of diverse cultural elites operating over four generations in that inland capital. Happily transcending the notion that philanthropy was moved simply by economic self-aggrandizement combined with a compulsion to control the behavior of less fortunate classes (a reading so often made by historians of welfare in recent years), she leaves room for the operation of authentic religious forces and for the impulse of civic stewardship as well as for class interest and social control.

Her analysis focuses on the creation and nurturing of institutions for the promotion of family welfare, public health, and cultural enrichment (historical societies, museums of art and science, libraries, and schools). Her protagonists include such prominent figures as Mrs. Potter Palmer, William B. Ogden, Marshall Field, Julius Rosenthal, Philip D. Armour, Richard T. Crane, Anita McCormick Blaine, Louise de Koven Bowen, and Jane Addams together with a host of friendly visitors and visiting nurses, volunteers, and board members whose fortunes were more modest and names less notable.

Especially useful are her analyses of major themes as they shifted over time: the different and changing roles of women and of men in initiating, sustaining, and managing charitable agencies and cultural institutions; the elaboration of new perceptions of poverty and strategies for its amelioration; the emergence of professional fund raisers and institutional managers in every field of endeavor; the consequent decline of the participating volunteer; and always the processes by which each generation sought to redefine "civic stewardship."

Although her interpretations might not fit perfectly parallel developments in other cities, larger and smaller and in other regions, hers is surely more than a narrow case study. Based on a critical reading of the secondary literature and on extensive research in primary materials, it ranges widely and deeply over concerns of fundamental national significance. Students of welfare and philanthropy and

of social elites, historians of ideas and of social movements, women's historians and urban historians will be rewarded by a careful reading of *Noblesse Oblige*.

CLARKE A. CHAMBERS
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ROSALIND ROSENBERG. *Beyond Separate Spheres: Intellectual Roots of Modern Feminism*. New Haven: Yale University Press. 1982. Pp. xxii, 288. \$19.95.

In the history of feminism two alternating theories have led to different strategies: either women are in some ways better than men—more ethical, more nurturant—in which case society needs to make use of these special talents; or else women's brains and abilities, except for physical strength, are just like men's, in which case justice demands simply equal treatment.

In America, starting about 1890, there was a switch from the first of these points of view to the second, led by a remarkable group of women scholars. Most of them were the first women to undertake graduate study at the universities of Chicago, Columbia, and Cornell. Rosalind Rosenberg, now on the Columbia faculty, tells the story of these women and of their sympathetic male professors. Her excellent study details what a rich intellectual climate such men as John Dewey, George Herbert Mead, W. I. Thomas, and Franz Boas brought into their institutions—the most exciting ideas of the day in philosophy, sociology, and psychology. Their women students eagerly applied these new tools to the specific problems of their sex, and while producing first-class research they showed by their personal commitment that women were fully competent in these fields. The first one mentioned is Marion Talbot, called to a deanship when the University of Chicago opened in 1892, where she had to figure out how to fit women into the academic structure. Soon Helen Thompson undertook the first objective comparison of the mental abilities of men and women. Other fascinating women took up the search, until the story concludes with Margaret Mead, who traveled the world over to discover what social meanings different cultures have bestowed on the words "masculine" and "feminine."

This body of work pretty well smashed the old stereotypes of women's congenital inferiority and also, Rosenberg thinks, of their special talents. Occasionally, but not often, her feminist sympathies may carry her too far, as when she describes Marion Talbot's departure from Boston as a flight from stifling conformity. In contrast, *Notable American Women*, the biographical dictionary, describes her environment there as liberal, with feminist sympa-

thies and tolerance of eccentricity, so Talbot's departure seems more like a missionary voyage than a flight.

Rosenberg is also aware that much recent biological research tends to point once again to sexual differences—in brain structure, in hormonal balance, and in evolutionary survival—but she resists this work as part of the age-old effort to keep women down. The subject deserves more respectful attention. It is possible that real sexual differences (if not "separate spheres") might enrich human life in ways not humiliating to either sex. The whirligig of time, Shakespeare tells us, brings in its revenges. Sure enough, an article in the September issue of *Science* 82, reviewing what biology and psychology has found out about the sexes in the last ten years, suggests anew that this information implies special ethical insights in women. Its conclusion: "Serious disarmament may ultimately necessitate an increase of women in government." Just what the early, pre-1890 feminists believed!

PRISCILLA ROBERTSON
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ALTINA L. WALLER. *Reverend Beecher and Mrs. Tilton: Sex and Class in Victorian America*. Amherst: University of Massachusetts Press. 1982. Pp. xiii, 177. \$15.00.

Altina L. Waller's analysis of the Beecher-Tilton adultery trial in 1875 is reminiscent of Clifford Geertz's analysis of the Balinese cockfight. It takes this ritual event (the "acted public document") and submits it to an exhaustive cultural analysis in terms of the participants, the crowd, and the social institutions that find expression through the ordeal by combat. Waller, however, is a historian interested in the dynamics of social change, and for her the Brooklynese cockfight constitutes a turning point in American history. Her detailed analysis makes a great deal of sense both as "the Jonesville [Brooklyn]-is-the-U.S.A. 'microcosmic' model and the Easter-Island [the Gospel of Love]-is-a-testing-case 'natural experiment' model"—to use Geertz's terms. Brooklyn represents for Waller both the socially striving middle-class Protestants at war with the Irish ward bosses and the conflict between the nouveaux riches (at Beecher's Plymouth Church) and the old rich (at Richard Storr's Pilgrim Church). Waller provides a fascinating interpretation of the ecclesiastical rivalry of the two congregations and of the relationship each had to Republican political reform in the years 1870–84. She also provides statistical analysis of the socioeconomic status of both congregations.

But the heart of her "thick description" of this "deep play" is her exegesis of "The Gospel of Love."

As Waller sees it, Beecher's theology was designed "to make sense and order out of a society that had experienced a shattering breakdown of social institutions" (p. 148) in the mad scramble for status, wealth, and power in mid-Victorian urban America. The Gospel of Love (her analysis is reminiscent of Emory Battis's theory about the Antinomian crisis in Puritan New England) was Beecher's belief that personal "affinity" of a higher spiritual sort was more important than contractual, institutional, or traditional forms of human relationships. In this case, the higher spiritual affinity between himself and Mrs. Tilton transcended their marriage vows and made right (even holy) what less-exalted and more old-fashioned moralists considered simply lust or passion. This same Gospel of Love also enabled the young, upwardly mobile clerks and entrepreneurs in Beecher's congregation to salve their guilt over shady business dealings, and it brought them closer, as reformers, to the local Irish ward boss who maintained that personal loyalties in politics were more important than ideological principles—all of which threatened and angered Beecher's high-toned enemies at the nearby Pilgrim Church. Beecher himself represents the transformation of evangelical religion from doctrinal orthodoxy and formal polity toward a religious "machine" with Beecher as its charismatic church "boss" (pp. 73, 145).

Waller probably overstates the case for "Brooklyn-as-the-U.S.A.," but she has certainly "unpacked" (as anthropologists say) much of the symbolic meaning of the Beecher-Tilton trial. This is a nicely wrought study, executed with meticulous care, and with great imaginative insight. It has much to tell cultural historians, women's historians, religious historians, and social historians.

WILLIAM G. MCLOUGHLIN
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NORMA BASCH. *In the Eyes of the Law: Women, Marriage, and Property in Nineteenth-Century New York*. Ithaca: Cornell University Press. 1982. Pp. 255. \$19.50.

In one of the most positive developments in recent historical writing, scholars have begun to break down barriers between disciplines and subfields. Intellectual, cultural, social, and political history have been blended in new ways by drawing on unexplored sources, adding a new texture to our picture of the American past. Norma Basch's *In The Eyes of the Law*, a study of the changes in laws pertaining to married women's property rights in nineteenth-century New York, is a good example of what an integrated approach can yield.

In The Eyes of the Law is much more than a

chronicle of legislative debates and the passage of statutes. The first part of the book is devoted to a lengthy and detailed analysis of coverture, a tradition imbedded in English common law that rested on the notion of marital unity. Under coverture, a wife was a "femme couverte" whose legal identity was merged to her husband's. A married woman could not own property, earn money of her own, make contracts, sue or be sued. Basch shows how the principle of marital unity was applied and reinforced in this country after the Revolution, in spite of the fact that it ran counter to republican ideology. She then explains how various pressures coalesced in New York between 1840 and 1860 to bring about changes in the laws. By 1860, New York had passed legislation that gave married women rights to their own property, as well as the ability to act on their own behalf in a variety of legal and economic endeavors.

Basch does not end there, with the passage of the laws. Rather, she follows these statutes through the courts to see how they were interpreted and enforced. She finds that the laws were weakened significantly by the judiciary and that notions of coverture survived long after the passage of the laws and indeed well into our own time.

The implications of this work are significant. First, Basch demonstrates how and why a group in power, in this case propertied men, would be willing to yield some power to those rendered legally powerless, in this case married women. Part of the motivation was self-interest based on the protection of family property. But the pressure applied by the women's rights movement also had an impact on lawmakers. At a time when reform was in the air, legislators found it increasingly difficult to justify married women's legal status when feminists pointed to the discrepancies between republican ideology and the doctrine of coverture. Secondly, the book emphasizes the critical gap between a law and its enforcement—something that scholars of the suffrage movement, the civil rights movement, and any other mobilization must continually confront. Lastly, Basch provides yet another corrective to earlier interpretations of nineteenth-century women's rights. Contemporary feminists were quite successful in pushing for changes in women's legal status, opening the door to suffrage and other significant gains for women in later years. There is no question that the pressure exerted by the women's movement helped lead to the passage of the laws. But it is equally true that there was no steady road toward progress and that entrenched attitudes remained entrenched. Basch also points to the pitfalls of a feminist movement comprised of middle-class women, which ultimately yielded gains that would benefit, for the most part, other women of property.

Although the text reads like a legal brief at times,

Basch makes excellent use of court records and legislative debates and fuses them to philosophical issues as well as popular ideologies pervading nineteenth-century America. For those concerned with legal history, women's history, or the complexities of political and legislative change, *In The Eyes of the Law* yields important new insights.

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TAMARA K. HAREVEN. *Family Time and Industrial Time: The Relationship between the Family and Work in a New England Industrial Community*. (Interdisciplinary Perspectives on Modern History.) New York: Cambridge University Press. 1982. Pp. xviii, 474. Cloth \$49.50, paper \$17.95.

Family Time and Industrial Time is a massive study of working-class family life and a major contribution to modern social history. Focusing exclusively on the Amoskeag textile mills in Manchester, New Hampshire, during the half-century prior to the 1930s, Tamara K. Hareven is critical of historical studies that have fostered a division between family and work. She effectively combines oral history interviews with former employees, company personnel files, and a growing literature on familial behavior to unravel subtle and complex ways in which families from rural Quebec confronted the demands of a large industrial complex. She makes an especially good case against a large body of sociological literature that limited the family as a cooperative unit to the earliest stages of industrialization and claimed it declined with the rise of specialization and new machinery.

Families found all kinds of useful functions to perform at Amoskeag. In addition to facilitating the recruitment of industrial workers by attracting kin, they provided instruction in work routines, informally established production goals, and allowed workers to cover for each other when breaks from industrial routines were needed. Kin arrangements even allowed older workers to keep their jobs longer, for they were able to trade positions requiring more speed with younger relatives more suited to such tasks. Hareven discovered that the ability of kinship to influence the factory system fluctuated with changes in the economy. When labor was in short supply kinship was most effective. During periods of reduced labor demand families were unable to exert as much leverage with employers.

Hareven also does a superior job in pointing out the various patterns of the household economy. While neglecting the process of establishing family priorities and the tensions that existed in attempting to get members to fulfill household roles, she does

catch the essential ingredient of the entire family economy: cooperation. Everyone contributed to the support of the household whether they were parents and children or boarders and lodgers. One French-Canadian recalled that even with nine or ten children "they're all gonna start working and pitch in."

Using statistical analysis as well as interview data on the entire life course of the families, Hareven deftly shows how familial responsibility, structure, and goals can change over time. She found that women's work, for instance, stopped not so much because of marriage but because of the stage of motherhood (p. 200). Indeed, middle-class ideals that censured the work of wives outside the home had not really penetrated immigrant families. Married immigrant women in Amoskeag were twice as likely to work as second generation or native-born women. Canadian-born women were more likely to work than married females from Eastern Europe, but perhaps this was due to the better connections the dominant French-Canadians had in the plant.

The reasons for changing family structures at different stages were not difficult to find. Family budgets were most solvent where a husband, wife, and at least one child worked; families were especially vulnerable in early stages when young children required a mother's care and prevented her employment. It was at this point that the income from boarders could help immeasurably. In all instances, immigrant households relied to a greater extent on the income of children than native-born workers who tended to be better off. The point is that familial "interdependence" was not simply a traditional trait but a reflexive behavioral pattern to low economic status.

Finally, Hareven argues that the "tranquility" of Amoskeag's family-worker enclaves was broken after 1920 when increased competition from Southern mills required demands for higher productivity and more frequent layoffs. She found skilled workers reacted strongly to these new demands and resorted to collective job actions to halt the drive for higher quotas in favor of a more reasonable working pace and an earlier standard of product quality.

In her description of the emergence of collective worker actions, she follows closely the arguments of several historians that later generations of industrial workers were less submissive than the first wave from preindustrial regions and more inclined to undertake collective worker action against bosses and the pace of work (pp. 149-51). This argument implies a dichotomy and an inevitable transition between early and later generations of industrial workers with the later group being more concerned with work place rather than familial issues and demonstrating a more developed sense of working-class consciousness. The problem with this argu-

ment is that it removes family concerns and the influence of familial discipline entirely from the content of working-class consciousness over time. This not only relegates family concerns to another era and culture but reduces the perspectives of workers entirely to work place issues. Such an argument is curious for Hareven since much of her scholarship is directed toward explaining the ties between family and work. In truth, they were never entirely separated among the industrial working class regardless of the generation. The fact that families could not exert as much influence after the creation of a central personnel office or scientific management schemes did not mean that family welfare and needs were forgotten; family needs were not dependent solely on the organization of work. While it is true that skilled workers vigorously resisted scientific management, Hareven does not really demonstrate that the mass of unskilled workers placed influence at the work place above familial need. What appears to be true is that both areas were of utmost concern to all workers and that familial discipline continued to exert as much of an influence as the discipline exerted on the production line. Despite all her excellent scholarship, Hareven, in fact, never gets inside the family to explore its system of discipline, priorities, and expectations of role fulfillment, issues that had as much of a contributing effect on workers' consciousness as those at work. But such criticisms should not detract from the overall magnitude of Hareven's accomplishment. *Family Time and Industrial Time* is quite simply required reading for anyone interested in the process of industrialization and group behavior.

JOHN BODNAR
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JUNE DRENNING HOLMQUIST, editor. *They Chose Minnesota: A Survey of the State's Ethnic Groups*. (Publications of the Minnesota Historical Society.) St. Paul: Minnesota Historical Society Press. 1981. Pp. xiii, 614. \$45.00.

This reference work is the collective product of specialists in the field of American immigration. The editor, June Drenning Holmquist, has engaged in the study of ethnic groups within the state of Minnesota for many years. For the initial inspiration for this publication she credits Carlton Qualey, well known for his expertise in immigration studies.

Covering the period from 1850 to 1980, the volume brings together pertinent information on more than sixty ethnic groups in thirty-two chapters, divided into four sections: (1) North Americans, including American Indians and blacks, (2)

Northern and Western Europeans, (3) Central and Southern Europeans, and (4) Middle Easterners and Asiatics. An appendix explains that source materials were gleaned from collections and publications of the Minnesota Historical Society, from the Immigrant History Research Center of the University of Minnesota, from federal and state census reports, from data provided by the Immigration and Naturalization Service, and from depositories and publications of ethnic historical societies. Many of the contributors made appropriate use of an indispensable recent work, the *Harvard Encyclopedia of American Ethnic Groups* (1980), edited by Stephan Thernstrom. Helpful tables, maps, and illustrations abound. Extensive notes appear at the close of each chapter. The writers emphasize people, folkways, and institutions, giving only minor attention to individual immigrants or their progeny.

While this volume contains contributions by seasoned scholars, promising younger men and women are also represented. All have conformed reasonably well to an agreed-upon format. Neither the melting pot theory nor the perspective of cultural pluralism is pursued. As a largely demographic publication, the volume provides much valuable data for historians and social scientists. Broad topics such as socialism and religion are accorded no special place in the index, but one can find references to social reform movements and to particular church denominations. Included generously in the detailed index, with multiple references, are such comprehensive topics as acculturation, benefit societies, discrimination, farming, marriages, newspapers, politics, and women.

To this reviewer it seems that the roles of the Germans and the Scandinavians might have been expanded, in view of their preponderant numbers in the state. In 1905 German-Americans totaled four hundred eighty-one thousand, including foreign-born and foreign-mixed, to use the terminology of the book. Swedish-Americans numbered three hundred eighty thousand in that year, and the Norwegians three hundred seventy-two thousand. Each of the three groups is given about thirty pages. By way of contrast, blacks in 1970 (1905 would be less meaningful in their case) totaled thirty-five thousand. In that year there were twenty-eight thousand Mexican-Americans, twenty-seven thousand Poles, sixty-six hundred Hungarians, thirty-six hundred Chinese, and forty-six hundred Japanese. Chapters on these smaller groups averaged twenty-seven pages.

Readers will appreciate this publication for its content and its readability. Implicit in much of the subject matter is more than local or state data. National and universal qualities and considerations are involved also. Understandably limited in scope to a single state, the book carries implications for all,

and for the countries of origin. As such, it offers a challenging pattern for a wider understanding and appreciation of the immigrant impact on American life.

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FRANK MORN. *"The Eye That Never Sleeps": A History of the Pinkerton National Detective Agency*. Bloomington: Indiana University Press. 1982. Pp. xi, 244. \$17.50.

Frank Morn conceptualized this carefully researched and thoroughly documented story of the first eighty years (1850–1930) of the Pinkerton National Detective Agency as a company history. He traces the formative years of its flamboyant and entrepreneurial founder, Allen Pinkerton, and describes the company's evolution from "detection to protection." In itself the story of the founder of this notoriously antilabor organization is somewhat ironic, for Pinkerton prided himself on his Chartist past in Scotland and on his aggressive abolitionist activities in Illinois.

Morn's underlying analysis has two themes: one the dynamics of corporate enterprise in the Gilded Age and after and the other the need of the rapidly growing nation for social control. The latter, he argues, outstripped the public response, thus creating the opportunity for private control organizations like Pinkerton's. The business history theme provides the better device for explaining Pinkerton's aggressive and shrewd conduct, as his publicity manipulation was the work of a genius. From his early and sensational exploits, like the foiling of an alleged assassination plot against Lincoln, to somewhat later ones like the exposure of the Molly Maguires, Pinkerton and his operatives alone provided the evidence of the "gigantic" plots that they had foiled. Were these plots real? Morn never critically takes a position. Whether real or not, Pinkerton did use the publicity to create an industry. And the industry visibly and sometimes successfully acted where no other social control organization did—in fighting strikes, using spies, and chasing professional criminals long distances.

In many activities the Pinkertons, even as the most honorable of detective companies, were unquestionably sleazy. The essence of their work involved the use of force or betrayal or duplicity. Although Allen Pinkerton used the language of the businesses that employed his services, his operatives could not be as honest as he claimed they were, and very often they had backgrounds similar to the thieves whose confidence they gained and betrayed. At the same time that Morn supplies enough evidence to make this basic aspect of the detective

businesses clear, his entrepreneurial framework obscures the shadiness.

Because the Pinkerton agency was the largest, best organized, and most image-conscious of all detective agencies, it was also the most vulnerable. Thus after the disastrous behavior of its operatives at Homestead, the company began to ease away from strikebreaking, concentrating instead on professional thieves, property protection, and employee "testing." By signing contracts with a jewelers association, a bankers association, and various railroads, the company prospered through the Progressive era.

Morn makes clear the distance between fictional detectives and the less glamorous "private army of capitalism," yet he also gives tantalizing hints of the ties between the Pinkertons and detective fiction writers. Allen Pinkerton and a team of ghost writers produced many books; his son befriended Arthur Conan Doyle; Dashiell Hammet worked as a Pinkerton operative for several years. These personal ties remind us that detectives used fiction in their daily lives and in their larger business development. They lied to suspects, made up fictional identities to gain the confidence of criminals, and used fiction to maintain the fear and trust of their clients. The blurring of the line between fiction and reality continues to plague the Pinkerton-like "stings" run by the FBI today. In any "sting" there exists a real temptation for its designers to offer high rewards. Only the experienced criminals will know that the rewards are suspiciously high and therefore stay away: the "sting" at once creates and catches the criminal. One imagines that the Pinkerton employee "tests" in the nineteenth century contained a built-in incentive to produce "criminals" in order to keep the lucrative contracts with the railroads. One wishes Morn had addressed these sensitive and difficult issues and explored their ambiguity more in this otherwise fascinating book.

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Los Angeles

JUDITH WALZER LEAVITT. *The Healthiest City: Milwaukee and the Politics of Health Reform*. Princeton: Princeton University Press. 1982. Pp. xvii, 294. \$22.50.

Judith Walzer Leavitt's book is local history of a high order and medical history of quality. Her concerns here are the public health problems of Milwaukee and the community responses to them between 1850 and 1930, the period of the city's greatest demographic and industrial growth. After analyzing the diseases that flourished in the unsanitary urban environment of this period, Leavitt traces the growth and political history of the city health de-

partment. She then examines the city's increasing involvement in such activities as infectious disease control, garbage and sewage disposal, and water and food protection. These sections are followed by an excellent and extended discussion of the rise of the city's numerous voluntary health agencies and of the effective twentieth-century relationship between them and the health department that enabled Milwaukee to earn a reputation as one of America's healthiest large cities.

The author presents her findings on these matters within a broad context of Milwaukee's political, social, and institutional history. One rewarding product of this approach is her analysis of the incidence of infectious disease among Milwaukee's ethnic groups and her perceptive discussion of German and Polish immigrant reactions to proposed public health measures. Of equal interest is her treatment of the role of health issues in bringing about a socialist city government between 1910 and 1912 and of the permanent influence of socialist ideology on the city's provisions for public health.

Leavitt writes clearly and without jargon in an attractive prose style. Her book is a careful and scholarly synthesis of a very large body of city documents, newspaper accounts, and other original material. It is essentially free of errors of fact or substance. Her organization of the material, however, has left considerable overlap and repetition. While most of this is not bothersome, the recapitulations of the final chapter should have been pruned substantially.

The one major shortcoming of the book is the author's failure to relate the Milwaukee experience at all consistently or effectively to the larger scene—state, national, and international. She makes no attempt to associate Milwaukee's politics with the Progressive movement at either the state or national levels. She does not provide adequate background information on the national and international scientific developments that were transforming the public health work of that period. She fails to acknowledge that the pioneering activities of earlier English, European, and American sanitarians must have influenced the development of Milwaukee's public health attitudes, institutions, and measures. And, finally, she does not begin to do justice to the influence of Wisconsin's State Board of Health. Many readers would have liked to learn something about early Wisconsin public health politics as it affected the local scene. And all would have benefited from at least some discussion of those areas of public health responsibility that were reserved to the state and of state activities in those areas, activities that affected Milwaukee's health as surely as did those of the purely local agencies.

JAMES H. CASSEDY
National Library of Medicine

EUGENE P. MOEHRING. *Public Works and the Patterns of Urban Real Estate Growth in Manhattan, 1835–1894*. New York: Arno Press. 1981. Pp. xi, 452. \$28.00.

An important development in both urban history and the history of technology is a concern with the development and impact of public works artifacts such as roads, sewers, water supply systems, and wharves as well as the delivery systems that provided urban services. Recent important studies in public works history include Carl W. Condit's monumental two volumes, *Chicago, 1910–1970* (1973, 1974), the American Public Works Association's encyclopedic *History of Public Works in the United States, 1776–1976* (1976), and the publications of scholars such as Louis Cain, Clay McShane, Martin Melosi, Jon Peterson, Harold L. Platt, and Mark Rose. To this list must now be added Eugene Moehring's study, *Public Works and the Patterns of Urban Real Estate Growth in Manhattan, 1835–1894*.

One of the chief virtues of this work is that it removes public works from the corruption-reform syndrome to which it had been relegated by many urban political historians and places it within the context of the city building process. Among the topics considered by the author are the development and impact of the Croton water system; the building of sewers and streets; the location and construction of police and fire stations, hospitals, markets, and schools; the construction of Central Park and its effects on uptown real estate; and, the building of the Brooklyn Bridge and its impact on the city. In addition, Moehring explores important questions such as the relationship between machine politics and urban development, the differential impact of public works on neighborhoods, and methods of labor supply and contracting.

Moehring thus examines a number of facets of the physical city that have largely been overlooked by historians in the past. Unfortunately, however, this coverage is uneven, and the manuscript often reads like a doctoral dissertation rather than a work of mature scholarship. The chapters on the waterfront, markets, and schools, and police, fire, and hospitals, for instance, are largely narrative description, sprinkled with judgmental statements. Little attempt is made to integrate the material tightly into a model of urban change or to use those of other urban scholars. On the other hand, Moehring's chapters on the impact of the Croton water system on the growth of Manhattan, of Central Park on uptown real estate, and of the Brooklyn Bridge on both Manhattan and Brooklyn, are insightful and illuminate the relationship between public works and real estate development. The material on sewers and gas lighting systems, also, supplies useful insights into the interaction of technological change and urban policy.

The author essentially argues, in contrast to many other urban scholars, that the privatistic approach to the city building process was the only one possible given the conditions of rapid nineteenth-century urban change. The main goal of public works, he observes, was "to stimulate local prosperity"; thus, "environment, aesthetics and human welfare were necessarily subordinate concerns. Real estate guided the infrastructure's development" (p. 367). In this context, Boss Tweed's regime was extremely significant, providing both institutional changes that facilitated the city's public works but also corruption and spoils. Eventually, concludes Moehring, although Tweed began more improvements than any previous administration, "suspicions of kickbacks and plunder did more harm than good," leading to extensive retrenchment in the coming decade (p. 324). From Moehring's pragmatic perspective, these developments were inevitable given the context of nineteenth-century urban growth.

In spite of its flaws, this book makes an important contribution to the literature on urban public works. It also presents a puzzle. In the foreword, the author thanks Arno Press for permitting him to publish his entire manuscript with a number of essential foldout maps. While the book contains many maps, however, they are poorly printed on single pages. In addition, the manuscript contains a number of typographical errors that should have been eliminated. The press has little to be proud of in the production of this volume.

JOEL A. TARR
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PETER J. BUCKLEY and BRIAN R. ROBERTS. *European Direct Investment in the U.S.A. before World War I*. New York: St. Martin's Press. 1982. Pp. xiv, 157. \$25.00.

In this small volume Peter J. Buckley and Brian R. Roberts outline the early history of European direct investment in the United States. Direct investment is defined as investment carrying with it at least some element of control of the satellite enterprise. In addition they attempt to place that history in the context of recent economic work and to analyze the successes and failures of the early multinationals.

The work represents almost no new basic research; however, the authors have brought together material from a wide variety of sources and they make it available in a quite useful fashion. In that process they provide quantitative estimates of the volume of direct investment and the industrial composition of that stream. They have also produced eight short case studies of European firms that did make direct investments in the United States. These eight studies are largely qualitative and drawn from the business history literature—a

literature all too infrequently exploited by economic historians. The eight include such well-known names as Lever Brothers, Royal Dutch Shell, Nobel, and Nestle. Finally, on the basis of their analysis of both the quantitative and the qualitative data, the authors provide some insightful generalizations about success and failure.

The volume will be read by every economic historian interested in foreign investment in the United States. The data presented exist in no other single source, and the authors are careful to provide a description of its limitations. In addition, their analysis of the decisions of the eight firms to move resources into the U.S. and their evaluations of those decisions make a useful addition to any study of the economic history of the period. The work should also have some appeal to the more general historian. It provides a useful (fairly rare) example of a successful attempt to wed quantitative and qualitative sources, and it provides an excellent example of comparative business history. All too often business history is highly idiosyncratic and provides little basis for our understanding of the "business process." Although not always successful, the authors have attempted to use business history as a laboratory to test current theories about the nature of the process of direct investment and the growth of multinationals.

LANCE E. DAVIS
California Institute of Technology

EDWARD WAGENKNECHT. *American Profile, 1900–1909*. Amherst: University of Massachusetts Press. 1982. Pp. viii, 365. Cloth \$22.50, paper \$10.00.

It is probably not often that a book is labeled as "charming" in the *American Historical Review*, but this is an appropriate description for the first of Edward Wagenknecht's projected three-volume history of the United States during his own boyhood years. In his career as a man of letters, Wagenknecht has written biographies of practically all of the major figures of nineteenth-century American literature. His *American Profile, 1900–1909* is a collection of miniportraits, gathered together between chapters on President Theodore Roosevelt's two administrations and introduced by a traditional survey course lecture that touches on some of the harshnesses of working-class life at the beginning of this century.

Chapters on "Representative Figures of the Time" tell the lives of William Jennings Bryan, J. P. Morgan, Andrew Carnegie, Thomas Edison, William Randolph Hearst, Lyman Abbott, Booker T. Washington, Jane Addams, Carry A. Nation, Helen Keller, Mark Twain, John Singer Sargent, Augustus Saint-Gaudens, Edward MacDowell, and David Belasco. Other, sometimes overlapping, chapters focus

on church and school, the fine arts, music, the press, magazines, and the theater. A trip through the kitchens of the food industry offers tasty tidbits from the sagas of Heinz 57 Varieties, Coca-Cola, Dr. Kellogg and C. W. Post, Campbell Soups, Uneeda Biscuits, and the Hershey Bar.

Born in 1900, Wagenknecht gives his portraits an inobtrusive touch of reality from his own memory of Jane Addams and of family breakfast table talk about the lovely, ill-fated Evelyn Nesbit. His Nesbit profile is portraiture at its best, but it also points up the weakness of Wagenknecht's undertaking. Nesbit's story shares conjunctive company with the great San Francisco earthquake and fire with no more continuity than two stories on the same newspaper page.

Images and impressions are no substitute for a point of view in serious writing, and no basic theme or inquiry, not even the Roosevelt presidency, illuminates the volume. *American Profile* does not touch any of the questions that have concerned this generation of American social and Progressive era historians. The result is not a profile of America but a collection of miniatures. They give pleasure, like a rich chocolate, selected after dinner from the sampler box, but it is difficult to enjoy very many of them at a sitting. The best recommendation is that Wagenknecht's profiles should be read, one a night, by all but busy historians.

DAVID CHALMERS
University of Florida

AILEEN S. KRADITOR. *The Radical Persuasion, 1890-1917: Aspects of the Intellectual History and the Historiography of Three American Radical Organizations*. Baton Rouge: Louisiana State University Press. 1981. Pp. viii, 381. Cloth \$37.50, paper \$12.95.

The Radical Persuasion, 1890-1917 by Aileen S. Kraditor is an ambitious revisionist monograph that seeks to challenge the central thrust of radical historiography for the past generation. It joins other recent works such as those by Bernard K. Johnpoll and Joseph R. Conlin in an apparent conservative trend as it attacks earlier historians' ideologies and methodologies, but it goes beyond its predecessors in scope. Kraditor attempts to clear historiographical debris and to start with a *tabula rasa* in explaining the history of the American left.

The author has written a topically organized, intellectual rather than institutional study of the Socialist Labor party, the Socialist party, and the Industrial Workers of the World from 1890 to 1917. Presupposing an extremely knowledgeable readership, Kraditor emphasizes American working-class cultures, claiming that historians of radical parties, in contrast to the new social historians, have failed to

do so. She argues that students of the left (including herself a decade ago) have ignored the workers' own milieus, have identified ideologically with the radicals they research, and have raised the wrong questions. She presents the historiography as monolithic with both Old and New Left historians operating in a traditional Marxist framework in which they impose their own a priori assumptions on their subjects.

Through a searing critique, Kraditor decimates the Sombartians who ask, "Why is there no socialism in the United States?" To question why workers have not turned to socialism, she correctly writes, is to assume that that was their logical path. Both the historians and the contemporary radicals approached workers with that expectation, and those biases hindered the work of each group. The historians concentrated on the perceived obstacles to socialism's success. The radicals, however, even had they discarded their blinders, would have failed to convert the masses because, the author insists, the ground was not fertile in that the workers were primarily loyal to their own belief systems.

Calling her era a "shake-up period" lacking a legitimate elite and therefore an integrated value system, Kraditor finds that the radicals nevertheless chose to assume the existence of a cardboard "System" against which to struggle. The vanguardism she notes in all radical organizations prevented these radicals from easily developing a relationship with the workers, whom they already misunderstood, other than that of leaders to would-be followers. Yet they were optimistic about the inevitable triumph of socialism based on so-called scientific facts. Certain theoretical givens became indisputable, but matters considered peripheral (rather than defining issues) because they were in the private sphere, such as race and the woman question, were open to debate in organizations that essentially bore the seeds of authoritarianism.

This book will have to be confronted by historians, but it is a flawed cry to battle. Marred by circularity in argument and by repetition, it is often injudicious in its statements. Unconvincingly, the author terms Eugene V. Debs unrealistic about American workers because of "self-selected" audiences. Radicals in general are damned whichever way they turn. For example, the Socialist Labor party and the Socialist party are excoriated for ignoring the economic needs of blacks, but the IWW, which did seek to meet those needs, is described as simply cultivating iconoclasm. The author's own historical inevitability shapes her whole thesis. The radicals couldn't win, the workers wouldn't listen, government repression was irrelevant, as was selection of tactics or the fact of leftist schism. Nothing would have changed the outline of events. Additional problems include textual inter-

ruptions due to the reproduction of documents. Kraditor's multitudinous sources demonstrate an unevenness. She claims to be building on numbers of documents never before quoted or cited (occasionally true), but her idiosyncratic choice of some sources and omission of others is not explained, and the footnotes, while very rich, are unnecessarily argumentative.

SALLY M. MILLER
University of the Pacific

FRANK TARIELLO, JR. *The Reconstruction of American Political Ideology, 1865–1917*. Charlottesville: University Press of Virginia. 1982. Pp. 200. \$20.00.

According to Frank Tariello, Jr., the United States was founded by men who believed in natural rights, limited government, and a society that was strictly a collection of individuals. He calls this "republicanism" or "free government." In the latter part of the nineteenth century, this set of beliefs was weakened by men, such as E. L. Godkin, William Graham Sumner, and others, who were ostensibly defending it. They were imprecise about key concepts, inconsistent, and ambiguous. When progressives like Dewey, Weyl, Croly, and other "collectivists" challenged republicanism the ideas were vulnerable. The collectivists "reified" society. That is, rather than see society as a collective noun, they saw it as something real in its own right and thus made it "fashionable to think in terms of the collective and not the individual" (p. 59). The pragmatists further eroded "free government" by denying the existence of all absolutes. Thus although they claimed they were defending individual rights they were in fact redefining them. These definers of the new individualism discovered that autocracy could come from sources other than government and that the state might in fact be used to counteract this supposed autocracy. They were thus led to a "denial of any right to property" (p. 117). In a brief conclusion, Tariello asserts that "the intellectual revolution . . . had a lasting and detrimental impact on American political thought . . . [I]t is quite clear that . . . a philosophical counterattack . . . is absolutely necessary."

In a historiographical introduction, Tariello mentions no book written in the last quarter century. He seems unaware that a major thrust of this voluminous body of work is to treat progressivism as either a fraud or a failure. If he had been, he would have had to meet the argument that wealth and power were distributed about the same way after as before his "reconstruction" and that the change was purely verbal.

He stays so exclusively on the ideological level that he in effect treats interest, whether economic, politi-

cal, or other, as irrelevant. Thus he cannot understand progressive skepticism about people who claimed to accept values such as freedom and social mobility, but were perhaps doing so only to protect their own interests.

This means he never takes seriously the progressive argument that tyranny could come from sources other than government. He scorns the progressive "discovery" of autocracy in the workplace but presents neither evidence nor argument to show why he rejects it. Similarly, he more or less accurately summarizes progressive views on why new circumstances required new techniques to preserve freedom but never says why he finds the argument unconvincing.

In fact Tariello wants a world of absolute and unchanging definitions: natural rights, private property, free government. His conclusion that America must return to true principles could be supported by his evidence. So could the opposite conclusion, or another conclusion entirely. Historians familiar with the literature of the field will gain little from this book, but historians studying the 1980s may find it of some significance that this book was published at this time.

DANIEL LEVINE
Bowdoin College

JOSEPH H. UDELSON. *The Great Television Race: A History of the American Television Industry, 1925–1941*. University: University of Alabama Press. 1982. Pp. xi, 197. \$18.95.

For more than a generation, media specialists, journalists, and social scientists have focused attention on television and its influence. On the other hand, historians have been slow to give serious study to the subject. One hopes this gap will soon be closed, if recent articles by Baughman and this monograph by Joseph H. Udelson are an indication.

The major emphasis of this work is on the formative era in the development of television. In his narrative Udelson skillfully details the interplay of technology, government policies, entrepreneurship, economic conditions, and market forces as well as the genius of individual personalities. The enormous expansion of television in the second half of the twentieth century was in part shaped by the men and women who laid the foundations of the industry in the two decades before World War II. Of course, the roots of television reach back into the nineteenth century. As early as 1873 Willoughby Smith, chief electrician of the Telegraph Construction and Maintenance Company, discovered the photosensitive properties of the chemical element, selenium. Five years later Alexander Graham Bell suggested a photophone (an instrument likely to be

more common in the 1990s). Between 1880 and 1900 a score of other inventors such as William Lucas, Paul Nipkow, and Jeanne Lazare Weiller, and also D. McFarlan Moore of General Electric, furthered the technical development of television. But the first successful public demonstration of television transmission was made in 1925 by C. Francis Jenkins—an authentic American tinkerer—in his studios in Washington, D.C. In the decade and a half thereafter, a variety of factors hampered the rapid growth of the industry. Rivalries between some of the inventors and potential marketers such as RCA and Dumont, the economic duress of the Great Depression, and the vagaries of the Federal Radio Commission and the Federal Communications Commission were among the influences that held up the emergence of television as a dominant mass medium. By 1941 the various participants had solved their major problems and were poised for a breakthrough. The Second World War delayed the long-awaited moment, but by 1950 the explosion of television was well under way. As Udelson's analysis indicates, however, television did not spring fully blown upon the American scene but matured slowly for more than half a century, particularly in the years from 1925 to 1941.

This well-researched book makes a useful contribution to the history of television. It fills a niche left void even by such magisterial works as Erik Barnouw's history of broadcasting. Udelson supplies a clear, compact analysis of the complexities that surrounded the early growth of the medium. In so doing he provides not only a competent narrative that provides insights into the various steps that led to the massive growth of the industry in the post-World War II decade but also suggests scores of still-unexplored topics to which historians can profitably turn their attention. Udelson's work helps point to directions that such inquiries might take and constitutes a worthy addition to the literature in this field.

GERALD D. NASH
University of New Mexico

CAROL E. JENSON. *The Network of Control: State Supreme Courts and State Security Statutes, 1920–1970*. (Contributions in Legal Studies, number 22.) Westport, Conn.: Greenwood Press. 1982. Pp. xiv, 205. \$27.50.

Carol E. Jenson's book deals with an important subject. The focus of the study is the treatment (and abuse) of First Amendment freedom-of-expression guarantees by state legislatures and courts, and the federal judiciary, particularly the U.S. Supreme Court. Periodically during periods of social stress between roughly 1920 and 1970 states have passed

and enforced a variety of security statutes. Formal designation of the statutes involved such pernicious-sounding activities as sedition, criminal syndicalism, and criminal anarchy. In practical terms, however, the laws, their application by local judges and juries, and their construction by courts of appeal (both state and federal) involved labor organizers, members of fringe political action groups such as Communists, individuals advocating civil rights and racial justice, or war resisters (particularly opponents of the Vietnam conflict).

Tracing the legislative and judicial record of these statutes, Jenson sets out a stimulating thesis. She argues that the laws and their construction by courts, taken together, represent a "network of control" that has been (and may be again) "tightened" to choke off a wide range of politically sensitive forms of speech and action. The study develops this thesis primarily through a case-by-case analysis of state and federal litigation. Principal attention is given to cases arising out of the post-World War I Red Scare, the labor unrest of the Great Depression era, the Cold War–McCarthy period, and the civil rights and anti-Vietnam struggles of the sixties.

In spite of numerous repetitions and resort to legal jargon, the book provides a useful survey of statutes and case law. Leading Supreme Court precedents such as *Gillow v. New York*, *Pennsylvania v. Nelson*, and *Brandenburg v. Ohio* are examined in detail. More than half of the study, however, explores the ways in which state tribunals have construed, distinguished, and often circumvented the Supreme Court's uneven but generally libertarian freedom-of-expression opinions. Paul Murphy, Zechariah Chafee, Jr., and other authorities discuss state judgments, but this is the first study of the interaction between federal and state jurisdictions that is book length. Throughout the discussion the author's deep concern for the potentially harmful impact of state policy on First Amendment guarantees and their Fourteenth Amendment extension is evident.

But a close reading of text and notes raises some difficult questions. Such a reading reveals that many (probably most) individuals found guilty under the "network" system eventually either had their convictions overturned on appeal (for example, from state to federal court) or avoided the full consequence of their sentence through resort to the political process (for example, a governor's pardon). Does this mean that despite the formal operation of the judiciary and the legislature, the ultimate function of the "system" was essentially symbolic? Or does it suggest that the effect of court decisions and statutes was too inconsistent and haphazard to constitute a "network"? Such questions do not limit the usefulness of this book for those interested in the evolution of

case law in a vitally important field. They do, however, show the need for a broader work approaching the subject in terms of both formal content and actual impact of law.

TONY FREYER
University of Alabama

ROGER A. BRUNS. *Knights of the Road: A Hobo History*. New York: Methuen. 1980. Pp. x, 214. \$10.95.

Although this book has some flaws, it will be virtually irresistible to anyone interested in hoboes. Roger A. Bruns, Director of Historical Publications at the National Archives, vividly portrays hobo life from the 1890s to the Great Depression. The hobo's golden era grew with the rapidly expanding railroad network and declined as farm and industrial mechanization cut the need for seasonal casual labor. Bruns sees the hobo as a knight, relative to the tramp and the bum. The hobo constitutes a working class of the road. Whereas the hobo was a "migratory worker," the tramp was a "migratory nonworker," and the bum was a "nonmigratory nonworker."

Bruns uses a variety of materials from the National Archives including investigative labor department reports and makes good use of the Library of Congress's Archive of Folksong for hobo songs. He also uses extensive interviews with veteran hoboes and hobo observers. Unfortunately the book is not footnoted and lacks an index. As the subtitle implies, this is not a history of hoboes, but a hobo history. The hobo appears largely as hoboes saw him. This is a good example of what New Left historians often call history from the bottom up.

Bruns capitalizes on the immediacy and vividness of actual hobo recollections, stories, jargon, and songs. Indeed, there is an extensive "Glossary of the Road" that translates hobo jargon for the reader. The anecdotal strength, however, becomes an organizational weakness. The book is so anecdotal that it might have been titled, "Nights on the Road." Of the eight chapters only four really treat a specific topic. The remainder (with titles such as "Not a Bum") portray hobo life with interchangeable, though consistently interesting, anecdotal material. The most useful chapters for historians of the period are the one that focuses on the hobo connection to the Industrial Workers of the World and another that depicts Chicago as the center for hobo culture. Most chapters exist in a historical vacuum. All in all Bruns makes an original and useful contribution to the history of the hobo and makes it with style and grace. Both the book and its selective bibliography are suggestive, if not definitive, works on the hobo.

JERRY RODNITZKY
University of Texas,
Arlington

JAMES E. SARGENT. *Roosevelt and the Hundred Days: Struggle for the Early New Deal*. (Modern American History.) New York: Garland. 1981. Pp. xi, 355. \$45.00.

This is a monograph about the First Hundred Days of the New Deal, with a focus unswervingly on personalities in Washington, D.C.—FDR and his advisors. James E. Sargent has a special attraction to the president-advisor relationship. The advisors are a familiar group: Moley, Tugwell, Douglas, Morgenthau, Warburg, Berle.

The events are also familiar. Advisors meet with the new president in this dramatic setting of national economic crisis, and they advise. The president listens to the advice and is somewhat shift. The Hundred Days takes place, the crisis eases. It still makes a good yarn.

The historian might ask what there is here that we do not have in print in Freidel's fourth volume, or in Rosen, indeed in Schlesinger. The reviewer must answer that the account given by Sargent offers very little new information, but it does present a somewhat critical summary of FDR's early leadership: "Roosevelt was working with available ideas, viewpoints, and personnel in order to restore and refurbish the mixed capitalistic economy at the public's expense" (p. 221). Sargent finds FDR "less liberal than many historical writers have purported" (the reviewer does not read these writers in the same way, but never mind), a man who "used casual methods, disorderly thinking, and ambiguous expression in his policy making and image making" (p. 264). This book is an account of those bad habits, and it seems to be written from what might be called the Moley point of view.

The research is a scrupulous sifting of familiar sources, a mighty labor that Sargent offers to a profession often too busy to replicate individual work in order to see if things still seem the way they were. His account may now be checked by those dubious about the work of the authors mentioned above and others who have treated this creative period of federal policy making.

Some might complain that there is not much fresh material here, but careful scholarship on even old problems has its value. Rosen promises us a study of the Second Hundred Days. I register the hope that his undeniable abilities might be directed, next time, toward more pressing historiographical issues.

OTIS L. GRAHAM, JR.
University of North Carolina,
Chapel Hill

ALAN BRINKLEY. *Voices of Protest: Huey Long, Father Coughlin, and the Great Depression*. New York: Alfred A. Knopf. 1982. Pp. xiii, 348. \$18.50.

This is an extremely well-written account of the rise to fame and influence of the two most colorful political agitators of the 1930s. As Alan Brinkley acknowledges in his preface, any attempt to evaluate the political careers of Huey Long and Father Coughlin is fraught with difficulties. Neither man left behind any personal papers of great consequence, forcing the scholar to depend on a great variety of peripheral manuscript collections and secondary sources. Further complicating matters are the controversial personalities and programs of both men that still embitter scholarly debate of their activities some fifty years later.

Undaunted, Brinkley plunged in anyway and has produced a very valuable study of the two men and their troubled followers. His major conclusion, as stated in the preface and on the jacket of the book, is that Huey Long and Father Coughlin were "manifestations of one of the most powerful impulses of the Great Depression, and of many decades of American life before it: the urge to defend the autonomy of the individual and the independence of the community against encroachments from the modern industrial state" (p. xi). The author develops this thesis brilliantly in chapter 7 entitled "The Dissident Ideology." Both Long and Coughlin despite their great differences in style and program, advocated that power should reside in the hands of the individual and the local community, not in the hands of the relatively few millionaires who controlled the great corporations and the government. The irony is that both men called for a powerful federal government to redress the balance in favor of the individual, yet both were ardent opponents of expanded federal power and constantly criticized Franklin Roosevelt and the New Deal for attempting to do many of the things that they themselves were supposedly advocating.

Brinkley dismisses both fascism and antisemitism as meaningful factors in the appeal that Long and Coughlin had for so many Americans of the depression era. He concedes that Coughlin was flagrantly antisemitic from 1938 on, but maintains that there is little evidence of public antisemitism in Coughlin or his followers before that time. As for the ever vexatious question of fascism, Brinkley states unequivocally that neither Coughlin nor Long were "fascists in any meaningful sense of the term" (p. 282). Instead, the author contends that "the Long and Coughlin movements may well have been the last effective expressions of the themes of populism in basic economic terms" (p. 168).

Voices of Protest is an important study that deserves a wide audience. Brinkley combines cogent political analysis with a gracious writing style that reminds one of Arthur Schlesinger, Jr., at his best. His research is very impressive; some 66 manuscript collections were used. While Brinkley does not

provide any important new biographical information about either Long or Coughlin, he does include some interesting new human interest material about their followers that he has culled from the Roosevelt and Hoover papers. T. Harry Williams's *Huey Long* remains the definitive study of the Louisiana senator, and Richard Davis's 1974 North Carolina dissertation, "Radio Priest: The Public Career of Father Charles Edward Coughlin," the most detailed study of the Royal Oak pastor, but Brinkley's incisive analysis of their political movements makes him the new authority in the field.

CHARLES J. TULL
Indiana University,
South Bend

DEAN L. MAY. *From New Deal to New Economics: The American Liberal Response to the Recession of 1937.* (Modern American History.) New York: Garland. 1981. Pp. xiv, 204. \$25.00.

The recession, which saw the loss, from the fall of 1937 to the spring of 1938, of two-thirds of the recovery achieved in the previous four years, occasioned a profound crisis for New Dealers. Events in Europe made it vital that America demonstrate the ability of a liberal democracy to solve economic problems and promote social justice. Meanwhile, the collapse raised questions that affected administration supporters' sense of personal worth. This self-doubt arose with the demise of the self-confidence that had developed with slow, steady recovery. Now, uncertainty about all past policies created an urgent need for a "program"—action, whatever form it was to take, as "part of an overall definition of just what the New Deal had been and where it was going" (p. 15).

Dean L. May delineates the clash that produced a policy that became a "program," a new liberalism. Henry Morgenthau, Jr., secretary of the treasury, and Marriner S. Eccles, chairman of the Federal Reserve Board, were the leading antagonists in "what Herbert Stein has called 'a struggle for the soul of FDR'" (p. xii). Morgenthau, who saw deficits as a cause of depression, held that the issue of philosophy was deeper than that of policy but nevertheless took a piecemeal approach to the problem, stressing various institutional reforms in accordance with the old liberalism. Eccles, who saw deficits as a result of depression, viewed the economy from a macroeconomic perspective, emphasizing a coordinated scheme of compensatory fiscal and monetary measures to influence the general level of economic activity without changing the structure of the economy or involving government in business decisions. Morgenthau, essentially a humanitarian, recommended policies that would limit

the funds available for relief, while Eccles, primarily an advocate of the immense productive capacity of liberal capitalism, proposed public employment of jobless depression victims. In mid-September 1937, Morgenthau began preparing a speech calling for the elimination of deficits through dissolution of the spending agencies. By mid-October there appeared signs of the slump that would set the stage for the adoption of Eccles's ideas on compensatory action as a standing policy.

In 1962, Robert L. Heilbroner stated, "In the end, it was not theory that settled the history of compensatory government spending, but history that settled the theory." May, in effect, concurs: "the enormous subsequent influence of the ideas of the *General Theory* [of John Maynard Keynes] was as much a consequence of the exigencies of that historical moment as of the elegance of the theory itself" (p. xi). But Heilbroner had in mind the impact of World War II, while May refers to the recession and the ensuing use of Keynesian analysis for theoretical confirmation of policies already decided on. May's fine study thus makes an important point concerning the chronology of the Keynesian revolution while supplementing the works of economists who have written extensively about the Roosevelt recession largely as an economic phenomenon rather than as a crisis for American liberalism.

BERNARD STERNESHER
Bowling Green State University

PETER H. IRONS. *The New Deal Lawyers*. Princeton: Princeton University Press. 1982. Pp. xiv, 351. \$19.50.

Somewhere in *War and Peace*, Tolstoy observes that the great battles are not decided by the general poring over his maps, but rather by the brave lad who seizes the regimental colors and calls on his comrades to follow. The thesis holds for law where forensic improvisations, often of necessity—the stress and deployment of facts, an idiom or turn of phrase in argument—may well determine the outcome of a case. Moreover, as Peter H. Irons early points out, the great cases of the United States Supreme Court do not go before that tribunal as *tabulae rasae* but on records that have been shaped at the trial and appellate levels by the tactical decisions of counsel.

Irons writes something of a history of the first New Deal in tracking its great cases from the statutory and regulatory drafting process through final argument before the U.S. Supreme Court. His net is wide and his vision panoramic as he weaves archival material and personal interviews into an impressive and exciting tapestry. Moreover, he brings one lawyerlike skill to the task—an ability to

compress a tangled set of facts and complex legal ideas into a terse and lucid précis that adds immeasurably to the pace and clarity of his narrative. Irons builds both the drama and the actors around three organic New Deal statutes and their paladins—the National Industrial Recovery Act, the Agricultural Adjustment Act, and the National Labor Relations Act. His names for the three companies of paladins concededly are more suggestive than descriptive—the legal politicians (NRA), the legal reformers (AAA), and the legal craftsmen (NLRA)—but they nonetheless admirably catch the style that politics and personality forced on the litigators. Moreover the drama is set in a context that includes the raw material of lawyering—forum shopping, case selection (and a point too often overlooked, case nonselection), bureaucratic squabbles and empire building.

To be sure, from Webster's federal refileing in *Dartmouth College* to the plea in abatement in *Dred Scott*, much of the stuff of American legal history is hidden in the interstices of procedure. But it has taken Irons both to suggest and demonstrate a new mode for putting it all together and doing so with a grace and acuity reminiscent of Bray Hammond whose Pulitzer Prize-winning *Banks and Politics in America* (1956) showed how, by selecting a fresh vantage point for perspective on a presumptively familiar terrain, the historian could make the lights and shadows of the past stand out in a way they never did before. But the study is not antiquarianism. In a compelling final chapter on the limits of legal liberalism, the author provides insights on contemporary concerns, which ironically enough attest the indelible stamp the New Deal lawyers left on their profession and their country.

GERALD T. DUNNE
St. Louis University

JOHN TOLAND. *Infamy: Pearl Harbor and Its Aftermath*. Garden City, N.Y.: Doubleday. 1982. Pp. xvi, 366. \$17.95.

GORDON W. PRANGE. *At Dawn We Slept: The Untold Story of Pearl Harbor*. Assisted by DONALD M. GOLDSTEIN and KATHERINE V. DILLON. New York: McGraw-Hill. 1981. Pp. xvi, 873. \$22.95.

Most American historians instruct their students that there are two views of the Pearl Harbor disaster, the revisionist one and the "correct" one. Then they examine the revisionist evidence and refute it with logic and fact. For students whose parents cannot even remember Pearl Harbor this has become just another sterile pedagogical exercise in need of scholarly transfusion and media attention to give it meaning.

These two books do the job. John Toland's work,

acclaimed by conservative ideologues who trace all of the nation's contemporary foreign and domestic troubles to Franklin Roosevelt, is warmed-over revisionism. Gordon W. Prange's book, the product of thirty-seven years research and the examination of all the recorded sources as well as extensive interviewing of the participants, provides the "correct" interpretation.

Toland argues that the Japanese-American war was an avoidable tragedy and that President Roosevelt and his inner circle knew in advance of the impending attack at Pearl Harbor. They refused to share the knowledge with U.S. commanders because they realized that only a Japanese attack would bring on the desired war. Roosevelt and his advisers thus engaged in an elaborate "charade," after which they arranged to place the blame on Admiral Kimmel and General Short.

In support of his revisionism, Toland offers "new" evidence. He mentions, for example, that on December 1, 1941, a radioman on the passenger ship *Lurline* picked up and later reported to U.S. intelligence a Japanese radio signal originating "North and West of Honolulu" that came from the attack force. A naval electronics expert in San Francisco, Toland says, also picked up such signals. In neither case, however, did U.S. policy makers receive the information. The problem with this evidence, as Prange demonstrates in his book, is that the Japanese strike force maintained silence and sent out no radio signals. Whatever the radiomen heard did not come from Yamamoto's force.

Toland also presents "new" information about U.S. knowledge of the Japanese winds code. He points out that Captain Laurence Safford, principal architect of the naval code-breaking procedure testified before one of the Pearl Harbor investigating committees that an associate had intercepted a Japanese winds message signaling war with the United States several days before Pearl Harbor. The associate, Ralph Briggs, also testified to this effect. The information, says Toland, was given to superiors to no avail.

It is true that the U.S. Navy learned of the winds code at the end of November, in which "East Wind Rain" was to mean war with the United States. But many officials testified that no Japanese message implementing the code was ever picked up, notwithstanding the testimony of Safford and Briggs. Moreover, Prange says that the two men were mistaken because what Briggs heard was in Morse code and the winds message was to be by voice. At any rate, since the United States already knew that "things were automatically" going to happen (meaning war) it is difficult to see how an interception of a winds message would have meant anything anyway, since it would have said nothing of the location of an attack.

Toland's most interesting discovery concerns evidence about an espionage mission to Hawaii of one Dusko Popov, a double agent in the employ of the Germans and the British. In the summer of 1941 German intelligence, at the behest of the Japanese, instructed Popov to find answers to a series of detailed questions about Pearl Harbor. Upon arrival in the United States Popov informed the FBI of his instructions and stated his belief that the Japanese would attack Pearl Harbor before the year was out. This information went to J. Edgar Hoover. Had it gone to President Roosevelt, it would help substantiate the revisionist charges. As an article in the *AHR* in December 1982 demonstrates, however, Hoover incredibly did not share this material either with the president or the military intelligence offices.

That Toland's evidence is questionable and his revisionism no more provable than that of his predecessors does not entirely invalidate his work. The book's primary focus is on the Pearl Harbor investigation, and it is an interesting and engagingly written account of the contest of wills, bruised egos, the soiled reputations, and all the human drama of the tragedy's aftermath. In that sense it constitutes a valuable addition to the literature.

But Prange's book is much better history. Originally thirty-five hundred pages in length, the manuscript was condensed and edited by two of Prange's students following his death. It is a carefully researched and copiously documented account of all dimensions of the attack. Fully half of the book's 738 pages deal with the planning and executing of the effort on the Japanese side, a record that in itself goes far in refuting the revisionist argument.

Prange attributes the tragedy as much to Japanese skill, determination, and precision as to American obtuseness, though he holds the military commanders in Hawaii, and particularly General Short, guilty of bad judgment. While policy makers were not blameless for the failure to communicate with Pearl Harbor, Prange says charges that Washington's messages were ambiguous are unfair. "The plain fact is that the War and Navy departments could not warn of Japanese action they themselves did not anticipate" (p. 411). He does not set out to dissect the revisionist argument (his thoughts are summarized in an appendix) but leaves little doubt that his research has uncovered nothing to substantiate it. Nor has anything altered the view that President Roosevelt's involvement in a conspiracy is manifestly illogical.

The book makes a number of important contributions. It dispels the notion that Pearl Harbor was a "supersecret" known to only a handful of planners in the Japanese navy. In fact, well over a hundred army and naval officers plus the emperor knew of the operation. It also identifies the originator of the idea of the attack and rejects the thesis that the

Japanese had initially developed the plan in the thirties. It tells the story of the activities of the espionage conducted in the Japanese consulate in Honolulu, and it provides a great deal of additional information and detail available nowhere else. If this is not the definitive account of Pearl Harbor, it will serve as the standard book for many years to come.

RUSSELL D. BUHITE
University of Oklahoma

ROBERT J. MCMAHON. *Colonialism and Cold War: The United States and the Struggle for Indonesian Independence, 1945–49*. Ithaca: Cornell University Press. 1981. Pp. 338. \$22.50.

This is the first substantial study of American policy toward the Indonesian nationalist movement and its struggle for independence in the years immediately following World War II. It is a scholarly monograph based on a wide range of American archival sources, most particularly the State Department and modern military records in the National Archives, on British cabinet and Foreign Office materials in the Public Record Office, and on United Nations records in the Dag Hammarskjöld Library. Robert J. McMahon is master of his materials and has produced a neatly written, nicely judged and well-argued work.

McMahon demonstrates that, despite the vocal opposition of the United States to European imperialism, such principles played only a very subsidiary and often muted part in shaping American policy toward the decolonization movements that emerged in Southeast Asia at the end of World II. Although, in the early years of the war, President Roosevelt had frequently expressed an unwillingness to see Indochina restored to the French and the East Indies to the Dutch, nevertheless when the Americans faced up to the burden involved in the final assault on Japan and the need for Allied cooperation in rebuilding Europe, they happily relinquished the task of re-occupying Southeast Asia to the British and so effectively washed their hands of the region and its problems. Consequently chapters 3 and 4, which deal with the surrender of the Japanese in the Netherlands East Indies and the efforts to secure a *modus vivendi* between the Dutch authorities and the Indonesian nationalists leading up to the Linggadjati Agreement of March 1947, are more about British than American policy. From the beginning the Americans tried to distance themselves from the problems, to appear to be a disinterested neutral while at the same time supplying the Dutch with the economic and military aid that enabled them to pursue their aims of regaining control of the Indies. As Stanley K. Hornbeck,

United States ambassador to Holland, put it, "We, in effect, attempted to support neither side and yet favored one and hoped not unduly to offend the other."

By 1947, however, this policy was coming unstuck. On the one hand the United States had come to believe under the Truman Doctrine that it was engaged in a global conflict with the Soviet Union and that therefore no part of the world was outside its ambit of interest. On the other hand the Dutch, by taking unilateral military and political action that violated their agreement with the nationalists, placed themselves in an indefensible position. Unwilling still to desert the Netherlands and wishing to obtain a solution in the Indies that would leave Western influence paramount, the United States rejected Australian and Russian appeals for the appointment of a United Nations arbitration commission and "manipulated the Security Council to serve its own policy objects" by having the council appoint a good office committee with very limited advisory powers.

It was not until early 1949, after the Dutch second "police" action had encountered extensive guerrilla resistance in Java and had been widely criticized both in the United States and United Nations, that the American government finally decided to review its attitude. The Americans recognized that the Dutch could not succeed, that their policy endangered the moves for Europe's economic recovery and for an Atlantic pact, and threatened to push all Asian nationalists into the arms of Moscow. Moreover, as Undersecretary of State Robert Lovett observed repeatedly, the Indonesian republicans were the best security against communist success in the East Indies. They had proved their anticommunist credentials; they were the only Asian nationalists to have suppressed a communist insurgency in their own ranks. Thus in February 1949 the United States forced the Netherlands' hand by threatening to cut off economic aid if the Dutch did not enter into negotiations with the Indonesians over terms for granting independence. The Dutch almost with relief conceded the point and in December handed over power to a sovereign Indonesian nation.

I have only two minor quarrels with the study. First it underplays the Australian role in the international diplomacy surrounding the Indonesian struggle for independence. McMahon does not seem to be aware of Margaret George's excellent study, *Australia and the Indonesian Revolution*, which gives a full account of Australia's distinctive regional perspective on the conflict and its attempts to influence American policy to achieve its aims. Secondly, the stress given in the introduction and conclusion to the significance of the "Open Door" policy goes much further than any of the evidence or argument

in the text itself would justify; it seems almost a ritualistic gesture.

These quibbles, however, detract little from the achievement, and if the State Department's historical section continues to recruit scholars of such caliber to its ranks we can remain assured that the U.S. foreign relations documentary series will remain in very good hands.

NEVILLE MEANEY
University of Sydney

STEPHEN G. RABE. *The Road to OPEC: United States Relations with Venezuela, 1919-1976*. (Texas Pan American Series.) Austin: University of Texas Press. 1982. Pp. ix, 262. \$25.00.

Most students of American diplomatic history are familiar with the Venezuelan-British Guiana boundary dispute and the Anglo-German blockade of the Venezuelan coast in retaliation for actions of Cipriano Castro; there is less familiarity with United States-Venezuelan relations beyond the early twentieth century. Stephen G. Rabe's new book should broaden this limited view by tracing events through the third quarter of the century and placing them in the context of the Open Door, the Good Neighbor, the Alliance for Progress, as well as the broader North-South debate. As the title *The Road to OPEC* suggests, oil provides the volume's continuity and major emphasis, but the author examines other important problems in U.S.-Venezuelan relations. Based primarily on American archival and manuscript materials, Rabe's research also includes British, German, and Venezuelan sources.

The author's conclusions offer few surprises. In the 1920s the United States was publicly committed to an Open Door policy but practiced a "Closed Door" policy in Venezuela. There was little analysis of the effect of United States capital and technology flowing into Venezuela. The supporters of U.S. investments as encouragement of growth, stability, and financial responsibility failed to ask who would benefit or for what purpose. Cordell Hull's reciprocal trade agreements were in essence close to Hughes's Open Door approach. While Presidents Truman and Eisenhower had distaste for military dictators, perceived Cold War needs influenced their friendly relations with such governments in Venezuela. Although Washington believed Venezuela was a successful example of the Alliance for Progress, Venezuela remained fundamentally a poor nation at the end of the 1960s. The author's purported differences with historian Bryce Wood over the meaning of the Venezuelan oil code of 1943 and whether Washington sacrificed U.S. business interest in helping arrange the law may be a little exaggerated.

More important in Rabe's story is the analysis of why United States-Venezuelan relations lacked major disturbances such as those marking U.S.-Mexican relations under similar conditions. Even between 1969 and 1976 when Venezuela no longer had to rely on U.S. investors or goodwill for its prosperity and when OPEC revealed its strength, there was no upheaval in relations beyond a few mutual recriminations. The author points out that into the 1970s Venezuela lacked technology and skill to operate and expand the extractive industries; Caracas feared disrupting investment and causing economic crisis. The Venezuelan political environment was moderate, and there were limitations on the nation's economic and political independence. Even nationalization of iron ore deposits and oil fields was relatively moderate, satisfactory to Washington, and not entirely displeasing to the companies. Venezuela chose to work within the capitalistic system. For this reason, as Rabe suggests, Washington's arrangement of affairs with Venezuela to adjust to world changes does not mean that a similar arrangement can be made with other developing nations.

This volume is based on extensive research, is well written, and presents measured judgments. It is a significant contribution to the literature of U.S.-Latin American relations.

WILLIAM KAMMAN
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BENSON LEE GRAYSON. *Saudi-American Relations*. Washington: University Press of America. 1982. Pp. iii, 163. Cloth \$19.75, paper \$9.25.

It has been said that America's main interests in the Middle East are oil and Israel—and that the two do not mix. *Saudi-American Relations* is a rather brief review of a complicated diplomacy by the United States to maintain a balance between the two and protect its economic and strategic predominance in the area. Until the Second World War, the United States viewed Saudi Arabia as a country that was more of a concern for the British. On June 8, 1943, Admiral Leahy, on behalf of the American Joint Chiefs of Staff, in a memorandum to President Roosevelt warned that America had an insufficient supply of domestic crude oil to meet the requirements of the armed forces and essential civilian needs. American oil companies already had concessions from the Saudi Arabian government and thus a new chapter in American-Saudi relations had begun. Progressively more political and economic involvement in Saudi Arabian affairs by the United States also caused a period of intense rivalry between America and Britain. But once the war was over the process of British withdrawal—politically

and militarily—from the Middle East had begun, and the obstacle to American expansionism from that base of power, had come to an end. American support for the state of Israel, however, constantly has tested Saudi patience, and the disastrous American performance in Iran, before, during, and after its revolution has undermined Saudi faith in America's ability or willingness to stand by the side of its avowed friends at the time of crisis.

Yet the United States and Saudi Arabia need each other's support. Saudi Arabia is the world's greatest supplier of oil and must maintain adequate supplies to keep prices within reasonable limits and help keep the industrial machinery of the Western world functioning. Hundreds of billions of dollars of American capital and profit are involved. For Saudi Arabia, fear of further Soviet influence in the Persian Gulf region, its concern for its own political stability, and the threat of radical Arab states as well as revolutionary Iran make necessary the U.S. umbrella of military protection, its military equipment and expertise.

Saudi-American Relations is basically a chronological rather than an analytical review. The author uses primarily the *Foreign Relations of the United States* and the *State Department Bulletins*, which accounts for the one-dimensional portrait of the subject. The primary sources used are limited, and thus there are some significant gaps in the narrative. It is hard, for example, to explain the omission of the Nixon doctrine and its profound implications both for Saudi Arabia as well as Iran—and the role of each for the security of the Persian Gulf region. One expects more substance in the study of a subject for which the author has exceptional qualifications both academically as well as professionally (including twenty-five years of service in the State Department, membership in the Royal Asian Society and International Institute for Strategic Studies).

NOSRATOLLAH RASSEKH
Lewis and Clark College

MICHAEL L. LAWSON. *Dammed Indians: The Pick-Sloan Plan and the Missouri River Sioux, 1944–1980*. Foreword by VINE DELORIA, JR. Norman: University of Oklahoma Press. 1982. Pp. xxvi, 261. \$19.95.

Dammed Indians, as the title suggests, is a study of the development and implementation of flood control projects along the Missouri River told from an admittedly Indian perspective. The bulk of the narrative in this volume consists of an account of two important events: the intergovernmental rivalries behind the adoption of the Pick-Sloan Plan for damming the river and the battles by Sioux peoples for compensation for flooded lands.

The Missouri River has often caused major flood

damage to communities along its banks. During World War II, Colonel Lewis Pick of the Army Corps of Engineers brought forward a plan to build a series of flood-control dams along the Missouri. Michael L. Lawson clearly has doubts about the engineering content of these proposals, but not about the political acumen with which they were presented. The Corps of Engineers' early introduction of a flood control plan allowed it to assume a commanding position relative to its rival, the Bureau of Reclamation, which produced its own plan designed by William Sloan, a year later. Despite interagency and interpersonal rivalries, the Corps of Engineers and the Bureau of Reclamation joined forces to stem a move in Congress to create an independent Missouri Valley Authority, modeled on the TVA. The Army Corps of Engineers dominated the implementation of the Pick-Sloan Plan adopted by Congress.

In 1947 the Corps of Engineers began the process of condemning Indian lands that would be flooded with no provisions for recognizing Indian treaty rights. Often the lands condemned for the project were valuable bottom lands whose timber and water resources were important to the Indian way of life. Each tribe was forced to stand on its own in its negotiations, with the Bureau of Indian Affairs providing little effective support. The settlement of claims continued for over two decades in the courts and in Congress where, finally, some special reconstruction and rehabilitation grants were awarded. As the settlement process proceeded, it appears that Congress became more receptive to Indian claims and the tribes more skilled in pressing their claims. During this time much of the energy of the Indian peoples was channeled away from developing reservation resources into fighting for greater compensation and rebuilding the reservation communities.

When the author moves from his basic source materials the narrative is weaker, although he tries to remain cautious in his judgment. We are told at numerous points that the dams and lakes disrupted Indian communities. It would have been far more telling to have evidence from the people affected, perhaps through more interviews. Noted native American scholar and writer Vine Deloria rectifies this in part in his introduction by relating some of his own family history. I also would have liked more discussion of why Congress was so willing to go along with the Corp of Engineers' proposals and which political constituencies were served by the plan.

In evaluating the plan itself, the author at times gets caught up in trying to show us that the Pick-Sloan Plan was especially harmful to Indians by counting burdens twice or by setting an impossibly high criterion for evaluating the project. For example, it is stated that "[if] the Pick-Sloan Plan was truly

designed to be beneficial to the people of the Missouri Basin, then it should be of equal benefit to those people . . . who suffered most as a result of its implementation" (p. 180). Almost any flood control plan will provide more benefits to the people downstream (who gain the advantage of flood control) than the people upstream (who lose their homes). Equity requires that the people upstream receive "fair" compensation and consideration of their rights, not that they also receive equal benefits. The Corps of Engineers undoubtedly overstated the benefits that Indians would receive as a way of justifying the low level of compensation paid, but this is just another way of saying that the Indians were undercompensated. It is also stated that the flooding of the reservation was especially hard on Indians since they could not leave the reservation. While the disruption of the reservation communities undoubtedly caused great hardships, in fact some Indians have left to seek work or for other reasons and, indeed, according to the 1980 census, a majority of Indians now live in urban areas. In the end we are left feeling sure that the Pick-Sloan Plan was far too concerned with the long-run interests of the Army Corps of Engineers and too little with the overall costs to the people, especially Indian peoples, of the upper Missouri Basin. In order to fully answer some of the questions raised here, however, we will need a more technical measure of how the costs and benefits were distributed and a discussion of alternatives that might have reduced the harm to the people of the upper Missouri.

My caveats should not obscure the fact that this book makes a valuable contribution to our understanding of the continuing and often unhappy relationship between Indians and the federal government.

LEONARD A. CARLSON
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NICHOLAS C. PEROFF. *Menominee Drums: Tribal Termination and Restoration*. Norman: University of Oklahoma Press. 1982. Pp. xiii, 282. \$19.95.

Menominee Drums, written by a political scientist, is a case study of the federal policy-making process and the role of an elite governing body. The story of Menominee Indian termination and the loss of Indian recognition, the costly experience of Menominee County, and the struggle for and final victory of restoration are the basis for the study. Nicholas C. Peroff divides the policy-making process into five phases that he identifies as input, decision, implementation, performance, and impact. He relies heavily on the 1971 anthropological work of George and Louise Spindler, *Dreamers without Power: The Menominee Indians*, to divide the Menominee

Indians into an elite governing minority and a nonelite majority.

In phase one, input, Peroff attempts to show that efforts toward assimilation led to the federal policy change that favored termination and that gave rise to an elite governing minority among the Menominees. Improper "inputs" led Congress to assume that the Menominees were ready for termination. Phase two, decision, clearly shows that decision making was beyond the control of the Menominee people and concentrated in Congress. Considerable attention is given to the influential work of Senator Arthur Watkins and the congressional committees, but final passage of Public Law 399, Menominee termination, and its provisions are neglected. If this had been addressed, then, the implementation of the law could have shown that the urgency of Public Law 399 necessitated decisive action by an agency of the tribe, since the secretary of the interior had the option of surrendering Menominee property to a trustee of his choice if no plan were devised. Consequently, Menominee leadership, elite or otherwise, formed an advisory council out of necessity. In the implementation phase Peroff ignores the contribution made by the University of Wisconsin Advisory Committee, chaired by Professor Burton R. Fisher. The committee's papers are housed in the Wisconsin State Historical Society archives. The options other than county status are not explored, but a good discussion of county status and the alternatives for the forest and milling industry are provided.

The book is best in describing the performance and impact of Menominee County and the DRUMS struggle for and achievement of restoration of the Menominee reservation. The concept of developing recreational facilities and tourism in the county was not a new idea from the Menominee Enterprises Incorporated group but had been discussed on the reservation as early as 1929 and again in 1947. The sale of Menominee land, as happened in the Lake of the Menominees project, was never a part of the earlier plans. Peroff freely criticizes the MEI programs and governance in Menominee County; he fails to show, however, how the reservation system of the past had built-in failure for any Indian-run plans in the future. Without properly trained leaders, teachers, engineers, nurses, and businessmen, the MEI assumed the operations of a county under termination and held the Menominee people together. Peroff is consistent in having DRUMS, described as a "new generation of well educated, politically sophisticated Menominees," become the new governing elite. While this reviewer is not as supportive of DRUMS as the author, their bid for restoration with self-determination is a credit to the Menominee people's resolve to preserve its tribal existence and Indian identity. Peroff's study focuses on a story without parallel in Indian history. It

provides a new dimension to the termination period with enough substance to satisfy both the historian and political scientist.

PATRICIA K. OURADA
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RONNIE DUGGER. *The Politician: The Life and Times of Lyndon Johnson, the Drive for Power, from the Frontier to Master of the Senate*. New York: W. W. Norton. 1982. Pp. 514. \$18.95.

Ronnie Dugger told Lyndon Johnson that he would write a book about him that was "fair, accurate, and worthy of the attention of serious people" (p. 402).

Almost every page of this book raises intellectual questions about fairness. Those who do not know Ronnie Dugger will wonder if he is telling the truth and why this sort of thing has not been revealed before. Those who know Dugger will not be surprised by the material but will ask how well Dugger has been able to manage his bias against Johnson.

There is much of Ronnie Dugger in this book, so it is helpful to know who he is. For years he has edited the *Texas Observer*, a liberal, statewide newspaper published in Austin. In the Johnson years, the *Observer* could always be counted on to expose the machinations of Texas politicians, especially Lyndon Johnson.

Frequently, *The Politician* takes on the tones of an *Observer* editorial, passing judgment on the misdeeds of Johnson, bemoaning the horrors of the nuclear age, and pointing out the sinister forces that have come into American government. Always, Lyndon Johnson is seen as the high priest of a venal new order. Dugger finds something tainted about even Johnson's most worthwhile accomplishments. He attributes Johnson's drive and hard work to insatiable political ambition. Every act is seen as another calculated move toward greater power. Dugger can see no more in Johnson's close relationship with Franklin D. Roosevelt than a whetting of Johnson's appetite to be president and Johnson's use of the friendship for leverage in Congress. Apparently, it is impossible for Dugger to imagine that Roosevelt might have taught Johnson something positive and good about the use of great political power.

One aspect that Dugger tends to overvalue is the influence of Johnson's rural, Southern-Western background. He believes that the Texas frontier tradition of taming the wilderness, civilizing savages, and taking revenge on enemies predestined Johnson to enter the war in Vietnam. There are two actions by Johnson that Dugger can never forgive: the Vietnam War and turning away from his liberal-populist heritage, which Dugger interprets as a loss of principles.

Another writer, using the same set of facts could produce a work much more sympathetic to Lyndon

Johnson, so it does not seem that Dugger has written a book that is fair in its judgments. There is, however, no reason to doubt the book's accuracy, especially in dealing with the minutia of political history. Much of the information comes from the hundreds of interviews done by Dugger and others. Dugger was given many long interviews by Johnson, and from these come many personal memories and details. In fact, there is probably more detail of the wheelings and dealings of Johnson in Texas and national politics than most readers will care to read. Surely, this must be the definitive treatment of the infamous story of Box 13, Duval County, in which LBJ allegedly stole the senatorial election of 1948.

It is because of its great richness of historical data that this book is worthy of the attention of serious people. If readers understand Ronnie Dugger's bias, they can look past it to get clear insights into the life and times of Lyndon Johnson. If they agree with Dugger's point of view, they will probably think the book a masterpiece.

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CANADA

COLIN READ. *The Rising in Western Upper Canada, 1837-8: The Duncombe Revolt and After*. Buffalo, N.Y.: University of Toronto Press. 1982. Pp. xii. 327. Cloth \$37.50, paper \$14.95.

The field of Upper Canadian history has lain rather fallow in recent years as Canadian historians, especially the younger ones, exploit the "new social history." The appearance of Colin Read's monograph, however, is not an interruption of the current trend, but rather an attempt to apply many of the techniques of the new methodology to a familiar setting.

The so-called Duncombe rising was an offshoot of the better known and more dangerous rebellion led by William Lyon Mackenzie in 1837 at the provincial capital. Although uncoordinated, both events derived from the growing discontent of a significant segment of the population. It has generally been assumed in the literature that the western rising, led by Dr. Charles Duncombe, was an expression of the frustration of the settlers of the area, who were American in background, over the restrictions and exclusiveness of a self-seeking Tory oligarchy at the capital. Read also attempts to deal with the oft-repeated charge that the rising was dealt with harshly and was followed by a thoroughgoing campaign by the Tories to extirpate all vestiges of reform.

With an imaginative use of sources, Read sets out to reconstruct a social profile of the rebels. The primary characteristics of definition that he employs

are religious denomination, national origin, political activity, and landholding patterns. Traditional sources—official manuscripts and newspapers—are given less weight than land records, judicial proceedings, census data, and economic indices. He concludes that the Duncombe rising was not a movement of the dispossessed. The rebels were often substantial, if not prosperous, settlers, usually American in origin and evangelical in religion, and many had formerly been involved in provincial politics. In other words, he has marshaled conclusive proof to substantiate what most writers had surmised. The obvious question, then, is about the utility of the exercise. Was it worth doing?

To some extent the study lacks focus. There are trees aplenty, but it is often difficult to discern the woods. Even Duncombe, the erstwhile leader of the revolt who fled to the United States, appears only fleetingly. We are presented with a bewildering succession of names with little coherent framework. On occasion, indeed, we seem to have little but lists. On the other hand, Read's research makes clear that there was no reign of terror in the aftermath of the rising.

There is another positive aspect as well. The author has shown that inventive research will permit a detailed analysis of early nineteenth-century society. Although in this case historical presumption has been proven accurate, it will not always be so. Read has left us with an excellent blueprint for further research.

J. E. REA
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LATIN AMERICA

DAVID PATRICK GEGGUS. *Slavery, War, and Revolution: the British Occupation of Saint Domingue, 1793–1798*. New York: Clarendon Press of Oxford University Press. 1982. Pp. ix, 492. \$67.00.

Britain's five-year struggle from 1793 to 1798 to occupy Saint Domingue is a largely forgotten episode in British history. In French and Haitian annals it is overshadowed by the French Revolution and the emergence of the Haitian Republic. David Patrick Geggus has now resurrected the affair and subjected it to a searching analysis, less ideological and more thorough than earlier treatments. His is the first work to study the occupation "in its totality" (p. 4), using all the available archival and secondary sources.

Geggus argues that foreign occupation of Saint Domingue in 1793 should be no surprise. Historians do not have to search for signs of Anglophilia or longstanding annexationist desires among Saint Domingue planters to explain their welcome of Britain. Self-preservation, not secession, was their

compelling motive. Geggus claims British designs on Saint Domingue, on the other hand, were "essentially aggressive and not inspired by fear for Jamaica's safety" (p. 87).

The invasion attempt was a costly failure, both in men and money, yet the occupation did play a key role in the transformation of Saint Domingue from Europe's wealthiest colony to America's first independent black republic. The general emancipation decree promulgated by Sonthonax at the end of August 1793 came as the British invasion loomed. Britain invaded to preserve plantation slavery and the Saint Domingue plantation society, yet, ironically, five years of constant warfare steadily undermined both. Geggus concludes that Britain by failing to suppress slave revolution in Saint Domingue increased its likelihood in Jamaica. But even more important, the occupation and the war accelerated the destruction of Saint Domingue's colonial plantation economy and, thereby, hastened "the most profound change then taking place in the Caribbean" (p. 388).

The war saw the emergence of Toussaint Louverture, the former slave, as the leading general opposing the British. He honed his brilliant generalship and molded a formidable army of former slaves during these five years of what Geggus terms both race and civil war. It was a war notorious for atrocities, although Geggus absolves the British by arguing that colonists on both sides were chiefly responsible.

Geggus is particularly adept at revealing the complexity of this plantation society in its death throes, especially in separate chapters dissecting white and slave society under military occupation. He also demonstrates that the plantation economy was surprisingly durable even during the British occupation. In a useful chapter on disease and mortality among British troops, he identifies the primary British enemy as yellow fever. His figures show that twelve thousand five hundred of an estimated twenty thousand five hundred British troops sent to the island died there, most from disease.

Geggus succeeds in peeling back layers of previous and often superficial historical interpretation. He challenges the conclusions of historians from J. W. Fortescue to C. L. R. James with a massive array of historical detail. This alone makes this work valuable, but in the process he manages as well to add significantly to our knowledge of the impact of the French Revolution on Saint Domingue and to illuminate the complex, shifting structure of Saint Domingue society buffeted by revolution and war.

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HOWARD J. WIARDA and MICHAEL J. KRYZANEK. *The Dominican Republic: A Caribbean Crucible*. (Westview

Profiles; Nations of Contemporary Latin America.) Boulder, Colo.: Westview Press. 1982. Pp. xv, 153. Cloth \$18.50, paper \$8.50.

Howard J. Wiarda and Michael J. Kryzanek have collaborated on a brief but informative and provocative introduction to the Dominican Republic. Ten chapters comprise this effort to provide "an up-to-date, comprehensive overview of Dominican national life" (p. xiv). A brief introductory chapter is followed by descriptions and analyses of the Dominican Republic's geography and culture, history, social structure, economy, political institutions, public policy, and international relations. The book ends with a very short set of summary conclusions. A map of the island nation, eighteen photographs, and a suggested reading list add to the presentation. As a whole, the book emphasizes recent affairs.

The authors argue that the Dominican Republic is a microcosm of change not only in Latin America, but in the entire Third World. Hence, it is the "crucible" of development-related issues. While this claim may be exaggerated, Wiarda and Kryzanek guide the reader through a number of often opposing phenomena associated with developing nations that are packed into the Dominican experience. They include, among others, despotic dictatorship and democratic achievements; foreign intervention, civil war, and relative stability; class and racial tensions and accommodation; and economic exploitation and progress. Their analysis stresses enormous changes wrought by Dominican modernization, especially during the past two decades. Wiarda and Kryzanek are sympathetic toward, but realistic about, the development process. They identify gratifying progress as well as fundamental problems in the polity, society, and economy. Their conclusions regarding the future are generally optimistic, based on recent political stability, but are tempered by awareness of serious economic structural flaws.

The book stands as the best general introduction to the Dominican Republic in English. It is a wide-ranging and balanced synthesis, based on the authors' extensive past research and personal observations. But specialists will also profit, beyond merely refreshing their knowledge about a country more important than its size, population, and resources would seem to warrant. Particularly original are the identification of competing domestic and foreign actors in the development process and the treatment of public policy and policy making. The book serves as a solid foundation for trying to understand a complex Caribbean nation.

G. POPE ATKINS

United States Naval Academy

DAVID J. WEBER. *The Mexican Frontier, 1821–1846: The American Southwest under Mexico*. (Histories of the

American Frontier.) Albuquerque: University of New Mexico Press. 1982. Pp. xxiv, 416. Cloth \$19.95, paper \$9.95.

David J. Weber's *The Mexican Frontier, 1821–1846* moves him away from his previous work, *Foreigners in Their Native Land: Historical Roots of the Mexican-American*, in which he sympathized with the victim, toward his present position, where he blames the victim. In his latest work, Weber describes Mexico's failures on the frontier that, according to him, caused discontent and alienation and forced the "frontiersmen" to ally themselves to the United States. His thesis is that "To Populate is to Govern" and Mexico's failure to populate resulted in its losing the borderlands.

In describing life on the frontier, Weber overemphasizes the point that the borderlands were sparsely populated. What is disturbing is that the author ignores the fact that all frontier regions are sparsely populated and that, in the case of Mexico, its boundaries resulted from three hundred years of Spanish imperialism. Once Mexico won its independence, like other Third World nations, it had to forge a national identity. In order to do this, Mexico needed time, which, in view of having the United States as a neighbor, it did not have.

The scope of the book is ambitious; it includes all of what is today's American Southwest and occasionally refers to northern Mexican states such as Sonora and Chihuahua. Weber meticulously describes life in this region, making thorough use of contemporary journals and reports and occasionally relying on archival materials. Weber's account of "The Collapse of the Missions" is solid; he puts secularization into a regional context, showing that the secularization that began in the eighteenth century was gradual. Other chapters are, however, uneven—his "Society and Culture," for example, while interesting, repeats the same flaws as the book itself: too many generalizations are made and Weber betrays a basic misunderstanding of Mexican regional history. He erroneously separates the culture of northern Mexico and the old borderlands from the rest of Mexico. This amounts to separating the American South or West from American culture. The author appears to ignore the fact that variations do not necessarily indicate separateness.

Moreover, Weber confuses concepts such as "internal colonialism" and "periphery" when referring to the borderlands. He also mixes apples with oranges in analyzing events. For example, on page 178, Weber draws an analogy between Anglo-American immigration into the United States in the 1830s and Mexican immigration into the United States in the 1970s and 1980s. This kind of misstatement gives credence to nativist charges that a Mexican invasion is underway.

Weber tends to romanticize the American and Mexican frontier experiences. He emphasizes their similarities when in reality they were markedly different, given their geographical and historical traditions. Weber also neglects the role of class in putting the "Frontier in Perspective." Lorenzo de Zavala and the oligarchy on the borderlands did not represent the region's thought. They represented a class of emerging Mexican capitalists, and their struggle reflected national politics, not local ones. What Weber evidently does not understand is that a restructuring of Mexican society was underway. And what alienated the borderlands was outside intervention—not a separate culture or even ideology. The Zavala, the Seguins, the Navarros, and their bunch must be viewed in the same light as men like James Wilkinson, Aaron Burr, and so on—men who represented their own self-interest.

In spite of criticisms of the author's interpretation and his apologist tone for the eventual takeover of the region, the book is not without merit. It is an important complement to John Francis Bannon's *Spanish Borderland Frontier, 1513–1821* and it provides a better understanding of life on the Mexican borderlands, which was far from idyllic.

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STUART F. VOSS. *On the Periphery of Nineteenth-Century Mexico: Sonora and Sinaloa, 1810–1877*. Tucson: University of Arizona Press. 1982. Pp. xv, 318. \$17.85.

This book is a detailed, complex, and richly suggestive political history of the Mexican Northwest. Its principal theme is the gradual integration over a century of an autonomous and peripheral region into the Mexican nation. In this process the region's politics, once characterized by family and town rivalries, became nationally oriented and institutionally based. It is an ambitious study, not only because of its time span (essentially 1767 to 1877), but also because it treats a colonial province and intendency that after 1831 became two states with somewhat different experiences. Moreover, while governmental and periodical sources for Sonora are plentiful, they are scant or nonexistent for Sinaloa. Though Stuart F. Voss generally surmounts these obstacles, the reader is at times hard pressed to sort out Sinaloan from Sonoran detail, and he inevitably gets a clearer sense of the latter.

Permeating the study is the intriguing question of the interplay between regional and national politics, a question on which the author shows ambivalence. Though he asserts at the outset that national politics depended in great part on resolving "the issues of

the how and the who of governance at the state and district level" (p. xii), much of his treatment demonstrates that the opposite was true. Even during the 1821 to 1854 period, local politics were influenced by the centralist-federalist conflict, though in an unexpected way. Federalism tended to encourage stronger (centralizing) regimes on the state level, centralism to undercut them. In Sonora, power shifted under centralism to the local notables of Alamos, Ures, and Arizpe. The centralist governor Manuel María Gándara even sought alliances with the Yaqui and Mayo Indians. In Sinaloa, always more sensitive than Sonora to national events, centralism brought the rise of the foreign merchants of Mazatlán and their clients. The state capital was even moved from Culiacán to Mazatlán. The pattern was repeated during the Reforma-Intervention years.

Nearly half the book is devoted to the decade 1867 to 1877, the critical period of national integration. Voss sees state (and perhaps also national) politics of these years as embodying the contradiction between liberalism and nationalism, between the professed principles of the Reforma and the Constitution of 1857 and the practice of arbitrary government and centralization of authority, which was increasingly necessary for national integration. A prime example of this contradiction was Governor Ignacio Pesqueira, an adherent of Benito Juárez and Sebastián Lerdo de Tejada, who dominated Sonora from 1856 to 1876. As a "national" liberal Pesqueira was opposed and finally overthrown by "local" liberals, who joined the movement led by Porfirio Díaz against Lerdo's authoritarian practices. A contradiction did exist, but is it not better understood as one within political liberalism itself? It was a contradiction made manifest, for example, in the nationwide conflict over the *Convocatoria* of 1867, the attempt by Juárez and Lerdo to write into the Constitution of 1857 certain centralist and interventionist features of the liberal dictatorship of the war years. Voss fails to mention the *Convocatoria*, but quite possibly the actions of Juárez and Lerdo (and their rationale) were analogous to those of Pesqueira on the state level. Liberalism became a national myth after 1867 and all aspiring politicians had to be liberals. The problem for the historian is how best to relate liberalism as a set of frequently contradictory ideas to the interplay of regional and national political forces. Voss identifies this formidable problem but does not resolve it.

CHARLES A. HALE
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G. M. JOSEPH. *Revolution from Without: Yucatán, Mexico, and the United States, 1880–1924*. (Cambridge Latin American Studies, number 42.) New York:

Cambridge University Press. 1982. Pp. xviii, 407. \$49.50.

This important book testifies to the vitality of Mexican revolutionary historiography. Scholars now recognize that the upheaval that struck Mexico after 1910 comprised a series of local movements, and efforts to explain them produced much of the best writing on twentieth-century Mexico in the last dozen years. This study expands the horizon, and it avoids the overly narrow focus of some otherwise solid regional studies by its careful attention to the interplay of local, national, and even international forces.

G. M. Joseph argues that conditions in Yucatan foreclosed the possibility of revolution from within. The once defiant Mayas had been reduced to a degraded proletariat, incapable of resistance. The state was in the grip of a handful of powerful families that controlled the cultivation and sale of henequen, a species of cactus native to the peninsula and used chiefly in the manufacture of binder twine. The planters' fortunes—and Yucatan's dependence on a foreign market—were assured at the turn of the century by the oligarchy's alliance with U.S. cordage manufacturers, in particular International Harvester, which established, in the author's words, an "informal empire" in the peninsula.

Given Yucatan's isolation from the rest of Mexico, it could be expected that a revolution there would be different, and indeed it was. The state escaped the turmoil that wracked other parts of the republic until 1915, when Venustiano Carranza dispatched an expedition to Merida to secure support for his embattled Constitutionalists and to appropriate tax revenues from the henequen export trade. Carranza's emissary, the onetime Sonora pharmacist Salvador Alvarado, decreed a host of reforms that included outlawing debt peonage and gambling, restricting the sway of the clergy, and opening education and government service to women. At the same time, he left intact the "Divine Caste" of henequen producers and the landholding system that supported them. Alvarado's goal, the author points out, was to increase productivity by removing feudal barriers and curbing immorality. He replaced the henequen brokers with a state regulatory agency, and his bold management, coupled with sky-high world prices for the "green gold," provided a bonanza for growers: "As the Revolution prospered," notes Joseph, "so did the planters" (p. 141). When Alvarado departed in 1918, his "bourgeois revolution" (as the author terms it) left the old order essentially in place.

Joseph praises Alvarado for breaking the private monopoly, but he reserves most of his enthusiasm for Felipe Carrillo Puerto, the home-grown zealot who was governor from 1922 to 1924. A romantic

dissident who professed a vague and unassimilated Marxism, Carrillo outdid Alvarado in social experimentation, instituting such bizarre innovations as "socialist" baptisms and weddings. He also pressed a campaign to mobilize the Yucatecan masses, with the evident intention of overturning the social order. But Carrillo's "socialist" revolution from above died aborning when he fell victim to a national counterrevolutionary movement whose agents in Yucatan captured and killed him, while his "leagues of resistance" were unable, or unwilling, to save him.

The revolution in Yucatan, possibly the most radical of Mexico's regional tumults, was over, except for an anticlimactic land reform in 1937 (also imposed from without) which the author portrays as a wrong-minded measure that permanently depressed the economy without measurably improving the lot of rural laborers. In the end, Yucatan's upheaval shared the fate of the others: it joined the all-encompassing "Institutional Revolution." Alvarado and Carrillo were inducted into the pantheon of national heroes, where they served to legitimize the postrevolutionary state ruled from Mexico City.

Not all readers will share the author's ideological premises or agree with his romantic conclusion that Carrillo Puerto, if given a chance, might have reversed the peninsula's unfortunate history. But "what might have been" is a matter of opinion. Joseph's exemplary research and sensitive analysis of what did occur guarantee this engrossing study a substantial place in the literature on Yucatan and the Mexican Revolution.

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JOSEFINA ZORAIDA VÁZQUEZ *et al.* *Ensayos sobre historia de la educación en México.* (Centro de Estudios Históricos.) Mexico City: El Colegio de México. 1981. Pp. vii, 234.

Education in Mexico has long been the subject of intense political and ideological debate. During the nineteenth century the National University was regularly suppressed and reopened as conservatives and liberals alternated in power. For much of the twentieth century, Marxism, sex education, and Catholic influences have divided educators, politicians, and the public alike. Until recently, however, historians have done little to relate the internal history of schools with the larger world of politics. We have the internal histories of specific institutions, and analyses of political and social aspects of education—the student movement, university autonomy, for example—but little in the way of synthesis.

In the present volume, four representatives of the current generation of Mexican historians attempt to

place development of schools and educational theory in their proper historical context. The four essays, published by the Seminario de Historia de Educación of the Colegio de México, portray education as both representative of culture and indicative of contrary visions of an alternative future.

Such relationships are, of course, complex. All four essays are successful in demonstrating how concrete institutional behavior conformed with a well-defined climate of opinion. Josefina Zoraida Vázquez tells the familiar story of early missionary efforts to teach the Indians Christianity, practical skills, and the fine arts as the outgrowth of dreams among Spanish humanists to provide some kind of universal education. The National University, on the other hand, represented the victory of tradition and orthodoxy by guaranteeing the survival of traditional thought among the *criollo* elite. Anne Staples traces the frustrating attempts of postindependence Mexico to establish an enlightened citizenry through universal education in the face of lingering elitism, avaricious foreign tutors, and economic and political chaos. Francisco Arce Guerra explains the educational controversies of the Calles years, an effort that appears less successful if only because Mexico of the 1920s lacked a coherent climate of opinion for education to reflect. He argues that the debates between "rationalists" (liberals?) and socialists, and between a Secretariat of Public Education intent on total domination and a variety of autonomous institutions from the National University to private schools can best be understood in terms of the desire to achieve economic modernization. Some readers may disagree with Arce Guerra's sympathetic treatment of Calles, if not his efforts to define any consensus at a time when individual ambitions and conflicting ideologies were completely at odds with each other.

Dorothy Tanck de Estrada, however, goes a significant step further in developing the relationship between institutions and their cultural milieu by showing conscious awareness on the part of the principal actors. In a provocative essay on the eighteenth century based on archival research, she argues that Mexican resistance to educational reforms imposed by Spain (secular institutions, Spanish language instruction for Indians, expulsion of the Jesuits) resulted not from intellectual opposition, but from incipient nationalism. Americans rejected the Spanish belief that Mexicans lacked the "capacity and integrity" to reform themselves, and realized that the reforms would lead to a diminished role for the indigenous clergy and fewer educational and career opportunities for sons of the elite. Members of Spanish scientific expeditions dismissed out of hand the contributions of their American colleagues.

Taken together, the four essays present a picture

of impressive projects and idealistic dreams constantly frustrated by political and economic realities. They also provide new insight into their respective periods by demonstrating the prominent role education has played. One hopes that the contributions of these historians will not be overlooked as such essays published in anthologies often are.

MICHAEL E. BURKE
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ANNA MACÍAS. *Against All Odds: The Feminist Movement in Mexico to 1940*. (Contributions in Women's Studies, number 30.) Westport, Conn.: Greenwood Press. 1982. Pp. xv, 195. \$27.50.

On the scanty shelf that holds works on women in Mexican history, Anna Macías's book is very welcome. It details how Mexican women struggled from 1890 to 1940 to reshape role, image, and opportunities for women to take their place as equal participants in society. The "odds" include vicious stereotyping (women become men when they undertake action in the public sphere; women's writing is praised by saying it is virile and wears pants), radicals who equated leisure-class women's emancipation with political ignorance or a vote for the church, congressional refusals to ratify legislation and seat women delegates, and the opposition of conservatives.

The most revealing chapters deal with women and profeminist governors Salvador Alvarado and Felipe Carrillo Puerto, who began to enact legislation and foster discussion to further women's political, economic, educational, and civic equality, as well as daring to open birth-control clinics and to talk about free love. The crucible of revolutionary politics in Yucatán did indeed promise a revolution for women.

Macías nicely complements Ward Morton's *Woman Suffrage in Mexico* (1962). Morton brings his story to a close with women receiving the municipal franchise in 1946, the congressional vote in 1954, and full political rights in 1958. Morton concentrates on national politics; Macías on conventions and the talk about broad social issues.

This is a good book and would be very useful for lectures on the Mexican Revolution. My only reservation is that the author has a tendency to pull the punches. "Feminism" is never defined. The ideology of equality in anarchism and socialism is not discussed, which makes it appear that men and women feminists in Yucatán sprang full-fledged from the limestone. Working women were praised in revolutionary Mexico for having political consciousness, but the work of women within peasant and labor organizations is not described. The achievement of Felipe Carrillo Puerto must be

"qualified," she says, because he convinced conservatives that feminism was dangerous. To ask a radical to dilute an issue is vain, and to suggest he might have succeeded if he had gagged his program seriously underestimates the opposition. The "antis" are not allocated a chapter, but they had clout. They organized demonstrations of women so effective that Cárdenas backed off from his support of sex education and socialist schooling, dumped the radical profeminist Francisco Mújica as his successor, and did nothing when his universally ratified woman suffrage amendment was stopped cold in Congress. One wonders if those mothers were accused of being men. This is a history with the extremes cut off. It is well worth reading, but the context of controversy is name-dropped, not developed.

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MANFRED MOLS. *Mexiko im 20. Jahrhundert: Politisches System, Regierungsprozess und politische Partizipation*. (Internationale Gegenwart, number 4.) Paderborn: Ferdinand Schöningh. 1981. Pp. 464.

Strictly speaking, this book is not a historical study, but a political and social analysis of Mexico, principally in the years since the Cardenas presidency. Its primary aim is to provide background for an understanding of what Manfred Mols considers the period of crisis Mexico has been undergoing since the early 1970s. Secondarily, Mols intends to use his book to advance suggestions on how the current threatening social, political, and economic situation in Mexico might be dealt with successfully. While *Mexiko im 20. Jahrhundert* is basically a work of political science, it uses history as a frame through which a fuller, more substantial and sophisticated understanding of the Mexican political and social system may be obtained. As a result, this study will be of great value to historians wishing to examine the results of those historical processes that shape the present.

The book is divided into twenty-seven chapters, grouped into six larger sections dedicated to Mexican history as a whole, the revolution and its institutionalization, economics, society, foreign relations, the structure of political participation, governmental organization, and the peculiarities of the Mexican political system. Each of these topics is considered judiciously and carefully, and Mols exhibits a remarkable familiarity with a wide range of material. Indeed, his extensive research (carried out in Mexico, Germany, and at Stanford University) and documentation are two of the book's most impressive features, and his notes and bibliography include an unusually extensive array of sources.

From a historian's perspective, there is little to criticize in Mols's work, basically because he relies heavily on the historical interpretations of the most respected modern scholars of Mexican history. As a result, historically there is nothing new or controversial in the book. Mols's observations and evaluations of more contemporary developments, those of the 1960s and 1970s, may be more open to criticism, however. This is true especially of many of his general comments on present-day Mexico and of his proffered solutions to the country's current problems. Mols views the Mexican Revolution as one of Latin America's great historic and political turning points, an event that has effected great positive change, but a movement now "distorted" to the point that it and the system it produced may no longer be salvageable. After outspoken criticism of the Institutional Revolutionary party, governmental corruption, bureaucratic inefficiency, and numerous other aspects of the Mexican governing establishment, he recommends in essence that the country live up to the Constitution of 1917 in reality as well as rhetorically, that only then can Mexico survive the dangerous times ahead.

This brief review cannot do Mols's work justice. It is a fine book in every way, thoughtful, concise, well documented, and knowledgeable. It can be read profitably by historians, political scientists, and the general public and should be translated into English and Spanish as soon as possible.

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Wichita State University

STEVE J. STERN. *Peru's Indian Peoples and the Challenge of Spanish Conquest: Huamanga to 1640*. Madison: University of Wisconsin Press. 1982. Pp. xix, 295. \$22.75.

This impressive book describes the process that in scarcely a century transformed the indigenous peoples of Andean Peru from members of relatively prosperous *ayllus*, communities, and ethnic groups under Incaic hegemony into an "Indian" proletariat, perceived as racially inferior, marked by poverty, and haunted by an uneasy sense that a vision of the world had been lost. The important highland region of Huamanga (Ayacucho) is the focus of this study, and in eight chapters Steve J. Stern analyzes the economic, social, and cultural forces that shaped its population from pre-Incaic times to 1640, a date that roughly marked the emergence in broad outline of the modern Andean peasant.

Chapter 1 sketches the subsistence-oriented way of life of the peoples of Huamanga, characterized by intricate reciprocity and ritual, and the delicate, deliberate appropriation by the Incas (after conquest in ca. 1460) of surplus labor to sustain the

larger endeavors of an expansive state. This process was interrupted by the Spanish in 1532, but, as chapters 2 and 3 demonstrate in exquisite detail, the new overlords, *encomendero* and church, copied often hazily understood Incaic practice and channeled tribute and labor demands through the native chiefs (*kurakas*), who thereby increased their own wealth and power. But this system of "post-Incaic alliances" could not long withstand the uncomprehending resentment of the *ayllus* and communities that bore its weight, peasants who, though respectful of Spanish military skill and the trappings of Catholicism, were unwilling to support the colonial superstructure with native resources. This resentment increased in the 1560s when the discovery of silver and mercury in Huamanga led to unprecedented demands for labor, especially at Huancavelica. Many natives in response embraced *Taki Onqoy*, a sect that called for a return to the old gods and a repudiation of things Spanish.

As chapter 4 demonstrates, the great Viceroy Francisco de Toledo averted crisis by asserting the authority of the state over Huamanga's natives; tribute amounts were standardized, the forced-labor system (*mita*) was established, and the rules between exploiter and exploited were codified, to be overseen in the future by *corregidores*. Simultaneously, the successful extirpation of *Taki Onqoy* was undertaken by the church. "In the 1570s," as Stern puts the matter (p. 80), "the local peoples of Huamanga finally became Indians," and the efforts of Toledo ensured prosperity for the elite until around 1600.

Chapters 5 and 6 meticulously discuss the decay and modification of the Toledan system in the four decades that followed. The period was marked by population decline, renewed Indian resistance to tribute and labor demands (especially the mining *mita*) expressed through flight and a skillful use of the law courts, Spanish encroachment on choice native lands, and a steady growth of more subtle labor recruitment devices to supplement the coercion of the *mita*. To oversimplify, the end result was that the cornerstone of Toledo's "Indian republic," the self-sufficient *ayllus*, underwent a steady, grinding erosion of resources that forced many natives, despite continuing access to land, to abandon the goal of self-sufficiency and enter the larger commercial economy to satisfy new-found "needs" by serving outsiders for wages or credits. In the process, an ancient way of life and its spiritual underpinnings were partially destroyed and nothing entirely satisfactory, not even the Christian God, was put in its place. Ironically, this fate, as chapter 7 vividly demonstrates, was felt most keenly by clever and successful natives who learned to play the colonial economic game and adopt Spanish ways, only to fall between two cultural stools, isolated from the communities of their ancestors and yet not quite accept-

ed by their Spanish models. As Stern concludes in chapter 8, all this has left its mark on modern Huamanga, where the most enduring colonial heritage remains precisely the challenge of conquest.

Stern gracefully acknowledges his intellectual debts at the beginning of this splendid account, most notably those to John Murra and Karen Spalding, but this remarkable book is very much his own. Based on extensive research in Peruvian and Spanish repositories, the complex interactions between the institutions and beliefs of two very different cultures are traced with notable perception and skill, especially the evocation of the experiences of the conquered. The book is a monument to both scholarship and comprehension, comparable in its treatment of the indigenous peoples after the conquest only to that of Charles Gibson for the Aztecs, and perhaps the best volume read by this reviewer in several years.

FREDERICK P. BOWSER
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NOBLE DAVID COOK. *Demographic Collapse: Indian Peru, 1520-1620*. (Cambridge Latin American Studies, number 41.) New York: Cambridge University Press. 1981. Pp. x, 310. \$37.50.

One of the fundamental points of departure for much of the new work appearing in the last decade or so on colonial Mexico has been the demographic base laid down by the Berkeley school of population historians, pre-eminent among them Sherburne F. Cook and Woodrow W. Borah. Until the appearance of the book reviewed here, however, no other similarly large region of colonial Latin America had received the attention of a careful historical demographer. Noble David Cook's work is not only of intrinsic interest but will also greatly facilitate the research of students of diverse aspects of the economic and social history of colonial Peru.

The book has two parts, of roughly equal length. The first addresses the question of the precontact size of the Peruvian population. Cook weighs in turn various methods that have been tried in the past to arrive at that figure—methods founded on ecological and archaeological considerations, depopulation ratios, standard rates of disease mortality, and social organization—and finds them all lacking. His constant criticism is that those who have used these methods have argued from excessively scanty information. His own approach is again the backward extension of later demographic trends; but he works from far more numerous data for the period 1570-1620 than anyone has gathered before, and also from scattered counts from earlier decades. The conclusion is that the population of Peru (within present boundaries), before any contact with

Europeans and their diseases occurred, lay in the range of 4–15 million. Rather tentatively Cook proposes 9 million as a plausible specific figure.

The second half of the book examines the demography of native Peru from 1520 to 1620 region by region. The largest block of information used is the general census taken by the fifth viceroy of Peru, Francisco de Toledo, in the early 1570s. Subsequent counts, irregular but numerous, were done of many districts. Cook uses them all. His efforts in finding, analyzing, and collating them have indeed been prodigious. Some of the conclusions he draws are expected, others not. For instance, as in Mexico, the rate of native depopulation in Peru was far higher on the coast than in the mountains. From 1570 to 1620 the highland population fell by 44 percent, while on the coast the decline was 65 percent. The reasons for this difference include less intense contact between Spaniards and Indians in the mountains and a lesser virulence of the diseases brought in by the conquerors in the highlands. As in Mexico, also, disease is given first place as the cause of mortality among the natives. So much more devastating was it than anything else the Spanish introduced that even in highland regions where Indians were forced into mining labor, their mortality was lower than that on the coast—an unexpected finding. Also unexpected is the contention that birth rates remained high among Indians, although offset by great infant mortality during numerous epidemics. Birth rates are often said to have fallen severely in Spanish America after the conquest.

By 1620 the native population of Peru had sunk to 670,000, probably no more than a tenth of its number in 1520, and possibly well under a tenth. The proportion of the decline, however dismal, will come as no surprise to colonial Latin Americanists: as in Mexico, so in Peru. We are grateful, nevertheless, to Cook for bringing his research of twenty years to conclusion.

PETER BAKEWELL
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H. E. MAUDE. *Slavers in Paradise: Peruvian Slave Trade in Polynesia, 1862–1864*. Stanford: Stanford University Press. 1981. Pp. xxiii, 244.

Within the growing body of literature on the history of labor recruitment in the Pacific, the mid-nineteenth-century exodus of islanders to Peru has received very little attention. In 1862 and 1863, some 3,470 Polynesians (including some Micronesians and Easter Islanders) were induced or forced into contracts for the plantations of a new state where slavery had been abolished, but where little had changed for unprotected Chinese, Negroes, and unfortunate recruits from the Pacific. The

numbers were not large, but as this detailed and well-documented study makes clear, the incidence was particularly severe for small populations of the northern Cooks and Tuvalu. Very few were repatriated after a mortality rate of over 90 percent—one of the highest in tropical labor history. Those recruits who were brought back in 1863, moreover, occasioned a second demographic disaster in the Marquesas, Easter Island, at Rapa, and in the Leeward Islands by introducing smallpox and dysentery.

Thus, from every point of view the episode is one of the most unsavory in Pacific migrant history, reflecting very little credit on the authorities at Lima or Callao and relieved only by the legal and naval action taken by the French administration at Tahiti and the efforts of Edmond de Lesseps, the French *chargé d'affaires* in Peru, whose persistence was instrumental in obtaining an end to the system.

The recruits died in droves, however, before the Peruvian administration acted. By a careful survey of official and printed sources collected and translated by a number of specialists, H. E. Maude has revealed both the full horror of their fate at sea and ashore in Peru, as well as correcting some of the demographic exaggeration that has accumulated in the works of earlier writers. It is now clear that most of the islanders were taken abroad by a mixture of trickery and deceit, while perhaps one-third of the total was taken by force. The large totals for the Cook Islands also include many who already had some experience of wage labor on Fanning Island, while those from Tongareva were familiar with labor migration to Tahiti. Thus, it is not quite accurate to conclude, as Maude does, that the initial cargo for Peru was so easily obtained “due to an accident” (p. 7) arising from a desire to spend cash on church construction. Rather, the recruitment can be seen from the evidence presented by the author as a particularly catastrophic incorporation of islanders into the cash economies of states on the Pacific border, at a date when few Europeans, except the French at Tahiti, were in a position to protect them.

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HERBERT S. KLEIN. *Bolivia: The Evolution of a Multi-Ethnic Society*. (Latin American Histories.) New York: Oxford University Press. 1982. Pp. xi, 318. \$6.95.

Drawing on the exponential growth of research during the past two decades, Herbert S. Klein has provided English-language readers with an admira-

ble summary of the sweep of Bolivian history from the Aymara kingdoms and Inca empire through the Spanish invasion and formation of colonial society, the genesis of the republic and neocolonialism of the nineteenth century, to the national revolution and neomilitarism of the recent past. The result is a volume that redresses Harold Osborne's observation that "there is no other country of this size which contributes so little to the general stock of information of the reasonably well-informed" (*Bolivia: A Land Divided* [1954]).

Particularly noteworthy is the author's presentation of the socioeconomic structure of the highland peoples. Rather than the generalities of earlier works, the reader finds a summation of John Murra's seminal conceptualization of the archipelago ("vertical integration of micro-ecological systems" [p. 18]), the Andean interregional-multiecollogical economy with its reciprocal exchange of goods, elaborate social stratification, and political hierarchy. The *ayllu*, enduring Andean kindred, is traced unerringly through the deformations of Spanish colonialism and the nineteenth-century onslaught of the capitalist world system.

The three chapters on nineteenth-century developments are to be recommended for their cogent exposition of the growth, crisis, recovery, and disintegration of the mining sector and the coming revolutionary storm. Klein's earlier work on these periods will be familiar to aficionados of Bolivian history.

A tendency of this, the most recent volume in the Oxford "Latin American Histories" series, to present material matter-of-factly gives the appearance of more historiographical consensus than may be the case. No doubt editorial guidelines dictated the streamlined presentation without footnotes; the result, however, is a source of frustration to the serious reader and a disservice to the initiate. Thus, for example, one finds the statement: "Land reform is not going to be challenged, nor is the Indian peasant going to be denied the vote" (p. 269). But as Paul Turovsky's recent thesis ("Bolivian Haciendas: Before and After the Revolution" [1980]) demonstrates, the agrarian reform of the national revolution is problematic. And what of the Indian vote? At best, the impression gained from the above is misleading: how is the peasant's vote counted? Certainly the MNR one-party system and the successive military governments since the fall of that regime have rested on coercion—only legitimized by the ballot.

A few minor quibbles with an otherwise laudable effort: Ché Guevara's Bolivian experience is trivialized as only preparatory to "other adventures," and reference to the "rebels" (or were they revolutionaries?) as "taken" by October 1967 ignores the continuation of the Army of National Liberation in the

urban *focos* of the late 1960s and their resistance to the Ovando and Banzer dictatorships.

Most susceptible to revision will be the opening chapter with its reliance on archaeological conventional wisdom: "In the highlands, Waru (*sic*) culture developed near Cuzco, and a major center appeared at the small town of Tiahuanaco just south of Lake Titicaca" (p. 13). Inca propaganda and successive Peruvian pundits have downplayed the level of *altiplano* socioeconomic development in the archaeological past while overstating the importance of the sierra northwest of the Titicaca basin. Andean archaeology is presently under reinterpretation, much of which will be viewed appreciatively by Bolivian nationalists who will hail the rediscovery of the *altiplano*—the largest uniform area of arable land in the Andes. Ongoing research by scholars at the Field Museum of Natural History finds the Titicaca basin the focal point for early cultural development, with Tiahuanaco the apogee of highland adaptation.

Klein's *Bolivia: The Evolution of a Multi-Ethnic Society* is, in sum, an excellent contribution to our understanding of that country's historical experience. A brief bibliographical essay at the end of the book points the way toward significant topical and historiographical directions; numerous maps and tables provide useful illustrations.

JAMES V. KOHL

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THOMAS C. BRUNEAU. *The Church in Brazil: The Politics of Religion*. (Institute of Latin American Studies, Latin American Monographs, number 56.) Austin: University of Texas Press. 1982. Pp. xvi, 237. \$27.00.

This book is a sequel to Thomas C. Bruneau's *The Political Transformation of the Brazilian Catholic Church* (1974) and, reflecting recent developments in Brazil, pays more attention than its predecessor to beliefs and practices of popular religion and how these concern the institutional church. Not only do contemporary forms of popular devotion have important implications for Brazil's social and political future, but they lead one to re-examine the country's whole ecclesiastical history since colonization. Bruneau starts off with such a survey, condensed but serviceable. Through the colonial period and nineteenth century, Brazilians were nominally Catholic but often lived beyond control and supervision by the somewhat feeble church apparatus. The 1891 constitution of Brazil's first republic separated church and state, leaving the former free to strengthen its ties to Rome and to expand its organization. This "Romanization" strategy was conservative, European, and oriented to urban, bourgeois society rather than to the rural populace, its

economic and social needs, and its unorthodox religiosity. When Getúlio Vargas came to power in 1930 with plans for a centralized state having corporatist and authoritarian features, church and state found ample basis for rapprochement.

Only in the freer political climate of the mid-1950s did the church begin to entertain ideological innovation. At this point the National Conference of Brazilian Bishops, representing a progressive sector of the hierarchy, and the lay organization Catholic Action both worked to reorient the church toward the common people. When the counterrevolution came in 1964, the church might, one supposes, have reverted to its traditional position. The fact that it did not, and the implications of this fact, are the author's central concerns. Not only, he points out, was the church's commitment to Vatican II difficult to abandon, but also it was soon clear that the church no longer enjoyed a favored position, that the state was withdrawing privileges extended under Vargas, and that the church was becoming a central focus of dissidence. What is more, the church hierarchy could no longer ignore rival claims to popular allegiance being made by Protestant sects and spiritist cults. An important response to non-Catholic movements has been its support for the Basic Christian Communities, of which some sixty thousand now exist. These partly spontaneous groups serve to replace the ineffective parish system, compensate for the chronic shortage of priests, and orient church interests and resources toward the poor. They in fact invert the traditional focus of the church and make resumption of that focus increasingly difficult.

The author supplements the straightforward presentation of his argument with survey data from eight representative dioceses chosen from Brazil's 222 ecclesiastical divisions. This sometimes opaque material tends to confirm rather than to supply primary evidence for the author's conclusions. Bruneau would have better served his readers (save for doubting Thomases) by suppressing the questionnaire material and enriching his historical and contemporary analysis from the "qualitative" sources to which he brings an informed and critical eye.

RICHARD M. MORSE
Stanford University

ALLEN WOLL. *A Functional Past: The Uses of History in Nineteenth-Century Chile*. Baton Rouge: Louisiana State University Press. 1982. Pp. 211. \$25.00.

It is a pity, that contemporary historians have to write books like this, revealing the dirty tricks of past historians. But nineteenth-century Chilean histori-

ans are vulnerable, and Allen Woll has written a stunning book that reveals the extent to which some of the most prestigious figures among them departed from norms of objectivity so as to serve political and personal causes.

The 1844 debate that pitted Andrés Bello, the Venezuelan-born father of university education in republican Chile, against his protégé, José Victorino Lastarria, is a landmark in South American historiography. Bello advocated the von Ranke approach to history. In contrast, Lastarria opted for the philosophy of history, which meant, as Woll describes it, that the facts should serve "merely as experimental data in the attempt to discover useful lessons for society or general laws of great value in the conduct of business and political affairs" (p. 32). Conventional wisdom has it that Bello won the debate and set the course for historical writing in Chile for at least the ensuing generation. Woll successfully challenges this view, showing that Lastarria and his associates earned at least a stand-off. From this time onward, Chileans approached history as a useful discipline that should serve a purpose. They found a variety of purposes to serve, ranging from progress, to defense (or defamation) of the clergy, of ancestors, and of rivals of ancestors.

Nineteenth-century debates over the secularization of Chilean society produced some of the most blatant examples of functional history. Defenders of the church, its claims and pretensions, depicted the institution only in the most favorable light, attributing the ills of colonial society to government harassment that prevented churchmen from achieving their always exalted objectives. Advocates of secularization, on the other hand, painted the activities of the colonial clergy only in tones of black, hoping thereby to facilitate the dawn of a new era in which the church would be stripped of temporal influence and subjected to government control even in its internal affairs. Functional history also flourished as historians turned their skills to establishing the truth about boundary disputes with neighboring republics. In several instances, Chilean historian-diplomats, beguiled by visions of Latin American unity, purposely ignored documentary evidence favorable to their country in its disputes with Argentina. In the vain hope of facilitating continental unity, they contributed to Argentina's acquisition of all of Patagonia.

Those about whom Woll writes include the Amunátegui brothers, Vicuña Mackenna, and Barros Arana, as well as a dozen or so lesser historians. Under their influence, the dye was cast. It was not surprising, then, that in 1973, the last year of Salvador Allende's rule, the government newspaper argued that historians were not performing a useful social function. It proposed "that the task of writing

history be taken from the university professors and given to those who were willing to use history to support Chile's incipient social revolution" (p. 1).

While he tells a disheartening story, Woll has written a book that can make historians proud of their craft.

FREDRICK B. PIKE
University of Notre Dame

NICHOLAS FRASER and MARYSA NAVARRO. *Eva Perón*. New York: W. W. Norton. 1980. Pp. 192. \$14.95.

In 1955 when President Juan Peron was overthrown by fellow officers and forced into exile, the body of Eva Peron, who died in 1952, disappeared, removed secretly by the new junta and later hidden in Europe. For nearly two decades Peron's followers clamored unsuccessfully for its return. Finally, it was removed from its hiding place, a cemetery in Milan, Italy, and given to Juan Peron who returned it to Argentina in 1973 when he went home from Spain to devote the last year of his life to being Argentina's president one more time. But it did not end there, for the myths remained, preserved and embellished by friends and foes of Peronism, and by dramatists who still found in her life the material of vivid, if sometimes sordid, theater. Largely ignored outside Argentina until recently, she has suddenly become the subject of a musical spectacular in London and New York, an American television film, and many hastily researched paperbacks written to take advantage of the new market.

Fortunately, a recent biography by Nicholas Fraser and Marysa Navarro does not belong among the latter. Rather, it is a serious attempt to demythologize a subject too long fictionalized by Peronists anxious to retain their popularity and denigrated by

opponents who erroneously believed that they could purge her from public consciousness. The work of a journalist and historian, this brief narrative is most appropriate for the general reader unfamiliar with Argentina. Lacking archives (most have been either destroyed or made inaccessible) or reliable autobiographies, they turned to interviews with Argentines once close to Peron, periodicals, and recent works by Argentine historians. Theirs is a disciplined inquiry that resists the temptation to praise or blame even when the Perons' behavior invites it. Especially helpful is the way they take a major event, describe existing controversies about Eva's role in it, and then tell what actually happened. We learn not only about the events themselves but also about how and why partisan beliefs developed.

After brief descriptions of Eva's youth and eventual success as a radio actress, Fraser and Navarro devote most of their attention to her political exploits, emphasizing her skill as a propagandist, and her ability to treat the unfortunate affectionately in exchange for the deference, and even reverence, that she coveted. One is reminded how popularity when joined by vanity provokes divided opinions about the ambitious among us. The authors are weakest at the end of the book when, instead of carefully taking apart the political mythology developed since her death and exploring its use by Peronists and their enemies, they hastily conclude their story with a brief summary of events in Argentina during the past twenty-five years. Nevertheless, this is a welcome contribution to the growing list of works on Eva Peron, one that should be read by the inquisitive before they turn to the more partisan accounts.

GARY W. WYNIA
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Collected Essays

These volumes, recently received in the AHR office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

ELLIS SANDOZ, editor. *Eric Voegelin's Approach to Thought: A Critical Appraisal*. Durham, N.C.: Duke University Press. 1982. Pp. xv, 208. \$24.75.

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R. B. OUTHWAITE, Problems and Perspectives in the History of Marriage. CHRISTOPHER N. L. BROOKE, Marriage and Society in the Central Middle Ages. MARTIN INGRAM, Spousals Litigation in the English Ecclesiastical Courts, c. 1350–1640. KATHLEEN M. DAVIES, Continuity and Change in Literary Advice on Marriage. VIVIEN BRODSKY ELLIOTT, Single Women in the London Marriage Market: Age, Status, and Mobility, 1598–1619. LLOYD BONFIELD, Marriage Settlements, 1660–1740: The Adoption of the Strict Settlement in Kent and Northamptonshire. ROGER LEE BROWN, The Rise and Fall of the Fleet Marriages. E. A. WRIGLEY, Marriage, Fertility, and Population Growth in Eighteenth-Century England. OLWEN H. HUFTON, Women, Work, and Marriage in Eighteenth-Century France. T. C. SMOUT, Scottish Marriage, Regular and Irregular, 1500–1940. L. A. CLARKSON, Marriage and Fertility in Nineteenth-Century Ireland. MIRIAM SLATER and PENINA M. GLAZER, Natural and Sacred Professions: Motherhood and Medicine in America.

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GERALD HOLTON, Einstein and the Shaping of Our Imagination. ARTHUR I. MILLER, The Special Relativity Theory: Einstein's Response to the Physics of 1905. PETER G. BERGMANN, The Quest for Unity: General Relativity and Unitary Field Theories. MARTIN J. KLEIN, Fluctuations and Statistical Physics in Einstein's Early Work. MAX JAMMER, Einstein and Quantum Physics. P. A. M. DIRAC, The Early Years of Relativity. BANESH HOFFMANN, Some Einstein Anomalies. LOREN R. GRAHAM, The Reception of Einstein's Ideas: Two Examples from Contrasting Political Cultures. ROMAN JAKOBSON, Einstein and the Science of Language. ERIK H. ERIKSON, Psychoanalytic Reflections on Einstein's Centenary. NATHAN ROTENSTREICH, Relativity and Relativism. YEHUDA ELKANA, The Myth of Simplicity. YARON EZRAHI, Einstein and the Light of Reason. ISAIAH BERLIN, Einstein and Israel. YITZHAK NAVON, On Einstein and the Presidency of Israel. URIEL TAL, Jewish and Universal Social Ethics in the Life and Thought of Albert Einstein. FRITZ STERN, Einstein's Germany. PAUL DOTY, Einstein and International Security. BERNARD T. FELD, Einstein and the Politics of Nuclear Weapons. PETER G. BERGMANN, Reminiscences. BANESH HOFFMANN, Reminiscences. ERNST G. STRAUS, Remi-

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Documents and Bibliographies

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LAVOISIER, ANTOINE LAURENT and PIERRE SIMON LAPLACE. *Memoir on Heat*. Translated and edited by HENRY GUERLAC. New York: Neal Watson Academic Publications. 1982. Pp. xviii, 43, 56. \$14.95.

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Communications

A communication will be considered only if it relates to an article or review published in this journal; publication is solely at the editors' discretion. Letters may not exceed seven hundred words for reviews and one thousand words for articles. They should be submitted in duplicate, typed double-spaced with wide margins, and headed "To the Editor."

TO THE EDITOR:

Robert M. Mennel begins his review of my *Oral History and Delinquency: The Rhetoric of Criminology* (AHR, 87 [1982]: 1360) by referring to the period covered in the book as beginning in 1800. This is inaccurate. The earliest instances I found of juvenile delinquents' oral histories were published in the 1840s, by Henry Mayhew and John Clay. My discussion of Mayhew and Clay comprises the first three chapters of the book. In the introduction I quote Karl Weintraub to claim that such literature would have been unlikely before 1800, but I cover this subject "since 1800" no more than I do since 1600.

According to Mennel, "Since his subject is participant-observer relationships, Bennett is particularly obligated to explicate his own point of view and to explore all the dimensions of his various topics." First, my subject is not "participant-observer relationships," which involve someone *participating* in an activity while also trying to be an observer, but rather oral history, which involves someone *listening* to someone else who has participated in some action. Those passages where I contrast participant observation to oral history, especially as they split apart in sociology after World War II, must have made no sense to Mennel. (Neither did Mennel notice my distinction between oral history and autobiography: analysis of an autobiography involves problems of plot and fully developed character that usually do not arise in oral histories. See my pages 4–5, as well as the rapidly-growing literature on autobiography, which contains almost no references to oral history. Thus, in limiting a potentially vast subject, I deliberately excluded books such as those

by Osborne, Behan, and Genet that Mennel complains I neglected, not to mention the "criminal autobiographies" of St. Augustine and Rousseau.)

Second, why should anyone be any more obligated to explicate a point of view or explore all dimensions in this subject than in any other? My rhetorical mode of analysis is spelled out in the book's introduction, and I cover a variety of aspects of this subject in sociology, political science, anthropology, psychiatry, journalism, religion, social work, rhetoric, philosophy, and literature. Criticizing someone for not exploring all possible dimensions of a subject is preposterous, unless the "dimension" is shown to be important and essential. But Mennel's "for example" in support of this statement relates to only an incidental point in my discussion of Mayhew and Alinsky.

If the implication is that one cannot write about oral history without giving one's own personal history, the same could be required of a reviewer of a book about oral history, an absurdity. But this does seem to be what Mennel has in mind when he accuses me of "reticence about [my] own position as program coordinator for the social sciences, Office of Sponsored Research, University of Illinois at Chicago Circle." First, I wrote the book before I took that position, so I could hardly be reticent about what did not yet exist. Second, this is a menial position locating funding for social science research, having nothing to do with "social policy analysts who are sympathetic to poor people." Even if these *ad hominem* remarks had been true, they would still be out of place.

Mennel accuses me of a "querulous tone" in "tiresomely berating" Mayhew for lacking Karl Marx's power as a social analyst. (Having warmed up on Mayhew, I can proceed to Mennel.) The book's index contains two entries for Marx, the relevant discussion on page 61 does no more than briefly make the point, and I conclude an admittedly ambivalent evaluation of Mayhew by praising him for *not* devising a social theory or program but remaining on the level of personal experience. Even on reflection I do not find anything querulous or

tiresome in that, but I do in Mennel's assertion: "Contemporary political pap, which utilizes isolated tales of welfare fraud to obscure massive transfers of wealth to the already wealthy, must be a special irritant."

Mennel ends his review by tacking on: "This book deserves to be widely read and discussed." But the review itself gives no reason for that recommendation. *Oral History and Delinquency* is one of the few books ever published about personal documents and only the second book-length study to appear during the past decade. (Paul Thompson's *The Voice of the Past* is the other.) It is the first attempt to do a case study of oral history by analyzing a series of examples of its use in relation to a single subject, beginning with the earliest examples. Mennel does not even inform his readers about the three questions that organize the book: Under what conditions (social, personal, technical, and rhetorical) have (or might) oral histories of juvenile delinquents come into being? How have (or do) they function? What have they accomplished? The book includes several major themes: the marginality of criminologists who have used such material, of the documents and their audiences, and of delinquents; the humanity of offenders, and the autonomy of local communities in programs of prevention and rehabilitation; the use of oral histories to promote various social and scientific "causes"; and the disregard by criminology of its own history and thus the repeated re-discoveries of oral history in criminology without a review of past accomplishments. Perhaps these could provide informed starting points for useful discussion.

JAMES BENNETT

University of Illinois at Chicago

TO THE EDITOR:

I am pleased that Elise Tipton found my book, *Organized Workers and Socialist Politics in Interwar Japan*, "interesting and useful" (*AHR*, 87 [1982]: 1144-45). That she delivered some criticisms in a rather caustic tone is not surprising; the subject of Japanese labor politics is a hot cauldron of interpretative controversy and many writers feel strongly about it. I was surprised, however, by the misleading nature of her criticisms, which left me wondering how carefully she had read the book.

Tipton says my approach was unobjective because I saw the Nihon Rōdō Sōdōmei as standing outside the mainstream of Japanese society and that therefore I unreasonably expected the Sōdōmei to "pursue policies of class confrontation and revolution." In fact, I wrote on page 4 that the labor movement under investigation must be seen as part of society, pointing out on page 5 that the dilemma of political

choice confronting the Sōdōmei and its labor allies was how to work in a peaceful and evolutionary way for progressive reforms from within the existing political order while avoiding cooptation by that order. During the 1920s the Sōdōmei coped with this dilemma by trying to make socialism compatible with nationalism in supporting (after 1926) a like-minded socialist party movement to advance the cause of the workers and of moderate social democracy generally. It is therefore absurd for Tipton to say that I expected the Sōdōmei to emphasize class confrontation and revolution. The chapter on the 1925 labor split should have made it plain to her that I expected nothing of the sort: the nationalist values of the Sōdōmei predisposed it to struggle against radical communism in the late 1920s and thereafter. But they did not preclude a reformist approach to socialism, reflected in the ideology and programs of the Sōdōmei's "realist socialism."

I felt that it was important and legitimate to ask why and how the Sōdōmei came to retreat from this commitment to socialist politics, breaking with the socialist party movement during the 1930s when the dilemma of political choice immobilized and overtook the mainstream of the labor movement, causing its final cooptation by its environment. Tipton is quite unfair and misleading to say that I did not explain this process of depoliticization because the book offers many explanations. Tipton need not agree with them. But she ought to have acknowledged their existence in a more balanced, objective, review.

For instance, she must have skipped chapter 4, which explains how the institutions and practices of industrial paternalism constricted the political activities of unions in large factories while pushing these unions into an increasingly cautious political stance. Tipton also ignores my analysis of how rivalries with communist unions on the left and ultranationalist unions on the right, and Sampō, intimidated and rendered politically passive organizations like the Sōdōmei in a climate of deepening national economic and political instability. Other explanations that eluded her attention dealt with the internal dynamics of labor's response to these and other challenges: the impact on labor politics of factionalism within the labor and socialist party movements, the political apathy of rank and file unionists, the growing bureaucratization of skilled worker labor elites, and so forth. Tipton chose to misunderstand my reference to Seidman's characterization of unions that helps to explain how hard it was for the Sōdōmei to overcome short-sighted preoccupations with economic goals in maintaining long-range political commitments and my discussion of how the timing of labor's development, especially in the context of rapid shifts from wage to job-competitive labor markets, kept the union movement small,

thereby making it more difficult (but not impossible) to sustain political initiatives.

Finally, Tipton implies my book is of the "praise and blame" genre and thus ignores my purpose, which was to clarify through this case study of labor politics the problems that beset an important social movement in interwar Japan and the dilemmas of political choice people faced in handling these problems.

STEPHEN S. LARGE
University of Adelaide

DR. TIPTON REPLIES:

I would like to thank the editors for giving me an opportunity to reply to Stephen Large's comment on my review of his recent book, as some of his remarks seem to deserve a response. My review began by saying that Large breaks new ground in his study of the nexus between labor unions and socialist parties in interwar Japan. This remains true, though it also remains true that I disagree with the book's methods and conclusions. The central conceptual weakness of the book is a tendency to impose an external value system on a historical situation, and then to judge the characters of individual historical persons in terms of their conformity to that system. This leads to an inconsistency of interpretation and to the book's failure to offer a convincing explanation of historical developments.

For instance, if the book had followed up the insight offered on page 4, that social protest movements are "an organic part of the context within which they operate," then it might have been in a better position to understand and explain actions identified on page 229 merely as "failure of vision and failure of nerve." However, the intervening chapters assume that labor unions should engage in political action aimed explicitly at the destruction of the social and political context within which they operate (see, for example, p. 171). The book thus imposes a daunting burden on the interwar Japanese labor union movement, arguing that if only the "fear-laden" leaders of the Sōdōmei had followed the lead of, say, Kikukawa Tadao, then somehow the "drift toward political authoritarianism and war" might have been slowed or reversed (pp. 164–65). The book argues they should have done so, despite the citation of comparative studies suggesting that labor unions always defend their institutional structure and focus on current issues in preference to pursuing long-term political goals, and despite the book's admission (p. 164) that in any event "it is unlikely that the Shakai Taishūtō [Socialist Masses Party] and a politically committed labor movement would have been able to stop this drift." In retrospect, from an abstract external perspective, it may

very well be correct that Sōdōmei leaders should have followed a more radical course. It may be true that "there was nothing noble about their compromises with militarism and authoritarianism" (p. 229). However, such criticism does little to advance our understanding of the choices actually made by individual Sōdōmei leaders.

Large's comment on the review rather dramatically reformulates the argument presented in the book, which clearly presupposes the desirability of a united proletarian party and blames the Sōdōmei for the failure of such a party to emerge. Despite its discussion of ideological factors in the Sōdōmei's choice of political alliances, the book concludes that "the Sōdōmei would not countenance a party which it was unable to dominate" (p. 111). Again, despite its discussion of ideological conflict in the 1925 labor split, the book concludes that the split resulted from the personal ambitions of middle- and upper-level Sōdōmei leaders (p. 70) while lamenting that the split "confounded attempts to organize a single united proletarian party" (p. 71). The comment repeats the book's tendency to judge rather than explain, imposing the requirement that the union movement avoid "co-optation" by the existing order—despite being an "organic part" of that same order—and assuming that emphasis on economic goals is "short-sighted" if it interferes with political action.

The book presents a large amount of material and many of the elements of an explanation of developments within the interwar Japanese labor union movement, but the elements are never integrated into a coherent structure, and the use of material is often incomplete. Repeatedly, the argument fails to build on and exploit insights, falling back instead on the tendency to judge and blame individuals for not conforming to a preconceived pattern of action. For example, the study by Joel Seidman cited in chapter 6 does indeed provide insight into why unions are preoccupied with economic goals at the expense of political commitments, but chapter 7 ignores this insight and instead merely criticizes union leaders for seeing the favorable change in the employment situation at the end of the depression "as a green light for pursuing nonpolitical sound unionism to strengthen union institutions in and for themselves, not as a positive development that might have strengthened the unions for political struggle" (p. 171). This was the point of the review—not that the book had misunderstood Seidman and others, or failed to mention various tensions within the labor movement, but that preconceptions of what ought to have happened had been allowed to obscure understanding of what in fact did happen.

Though suffering from serious shortcomings, the book may nonetheless prove useful to those interested in labor movements and the interwar period. As

always, readers will have to judge for themselves the adequacy of the arguments advanced.

ELISE K. TIPTON
Sydney, Australia

TO THE EDITOR:

William Stueck's review does my book, *The Unwanted Symbol*, and *AHR* readers a great disservice (*AHR*, 87 [1982]: 1195–96). He makes no mention of the theme, new discoveries, new interpretations. Instead, he overlooks the reality of East Asia to make rather minor comments about bureaucratic politics in Washington. Let me acquaint your readers with what I wrote.

I sought to explain the significance of Korea to American foreign policy in the period between the wars. My key point—and the subject of praise in nine, previous, favorable reviews—was that of the unwanted symbol. It implies tension, conflict, confusion. I sought to describe a process by which, unwittingly and unwillingly, the United States felt its prestige and commitment determined by the continued existence of a noncommunist South Korea. Ultimately, the book will rise or fall on the strength of that conception; Stueck fails to discuss it, even in passing.

I also made several new points, heretofore, never published. During the Second World War, Korean policy was delayed for fear of the impact on Britain's predicament with Indian nationalism. I found an exciting—at least coincidental—relationship between the two phases of the Berlin blockade in 1948 and a cutoff of electric power across the 38th parallel in Korea. Were the Soviets attempting a primitive sort of signaling? I explained the potential importance of the Pacific Pact between Chiang Kai-shek, Syngman Rhee, and Elpidio Quirino of the Philippines. Although the pact never materialized, it may have caused Mao, Stalin, and Kim to view the United States as seeking to organize an association of Asian dictators to threaten the communist rulers. I also pointed out the importance of the Korean Assistance Act of 1949 and, when it became caught up in the debate over the China debacle, how the administration's perceived need for its passage prevented it from disengaging from Chiang's discredited regime on Formosa via the Far Eastern Economic Assistance Act of 1950. None of these new points appear in the review. Also, I was among the first scholars to review the files of RG 84, Records of the US Mission to the United Nations, and, consequently, among the first to make much use of the Korean materials. I also made extensive use of Congressional documents including recently declassified hearings held in executive session in the 1940s and 1950s. Stueck also ignores this.

Permit me to comment on Stueck's criticisms. I am disappointed by what he attempted to do. First,

he overlooked what I had written: I backdated Britain's concern over its colonies—especially India—to the midpoint of the war, where it belongs; considering that the administration chose trusteeship for Korea—and dropped it elsewhere for colonial nations—it is more important to discuss the continuities between Roosevelt and Truman on Korea than the discontinuities. Korea became the prime example of Roosevelt's idea of trusteeship; of course, I spent a great deal of time on the army's problem with the strategic importance of Korea. The issue is far more complex than Stueck implies in his misdirected criticism. As he well knows, army leaders recognized the obvious strategic importance of Korea in the fulcrum of power in Northeast Asia. Clearly it was important to Japan. Still, the army had to balance the strategic importance of the peninsula against the nearly insolvable problem of its defense combined with declining military budgets. Korea was important but given limited resources until NSC 68; other areas figured more prominently. I also discussed interservice and interdepartmental and interbranch rivalries. As for my treatment of the months immediately prior to June 1950, Stueck is damning me for not covering in great detail that which I stated went beyond the scope of my study. I intended to provide a necessary background from which the debate over the origins of the Korean Civil War and our intervention could begin. His criticism would make a reasonable suggestion—which I declined to accept. Lastly, as for tracing our relationship with the Soviet Union back to the 1930s, several points need be made. That comment demonstrates Stueck's lack of understanding about East Asia. Also, where does one commence? 1917? The emergence to world power in the 1890s?

I wrote a solid account of the relations in, with, and about Korea between the wars. Constructive criticism of that account remains to be written in the *AHR*.

CHARLES M. DOBBS
Metropolitan State College

PROFESSOR STUECK REPLIES:

If Professor Dobb's modest recital of his book's contributions encourages scholars to read it for themselves, so much the better: I will soon have the reputation as one of the most charitable reviewers in the profession.

WILLIAM STUECK
University of Georgia

TO THE EDITOR:

James C. Foster's review of my *Class and Culture in Cold War America: A Rainbow at Midnight* (*AHR*, 87 [1982]: 1196) is neither accurate nor fair. He mis-

represents the content of the book and substitutes *ad hominem* attack for scholarly criticism.

Foster chides me for the "narrowness" of my research, specifically my neglect of the UAW records on the 1946 contract and the ACTU files on the expulsion of communists from the CIO. In fact, I do cite the ACTU files (twice on p. 242); but more to the point is the fact that those topics take up fewer than six pages and appear merely as background material for my primary argument. Foster fails to mention that I investigate shop floor conditions and worker strategies of independence on the local level, and that my research draws upon manuscript collections, oral histories, union records, government documents, personal correspondence, and newspapers from all parts of the country. Nor does he relate that my book provides the first comprehensive historical treatment of general strikes in five U.S. cities in 1946, of lengthy disputes over communist leadership in local unions in 1948 and 1949, and of the political nature of shop floor militancy throughout the decade.

By neglecting both my thesis and my evidence, Foster conceals the true concerns of the book. He belittles "the surprising (to the author) cases of labor solidarity in bitter local strikes" as an example of evidence that I choose more for ideological than historical importance. This passage actually refers to my accounts of general strikes, not just labor solidarity (pp. 56–86). In one year workers in cities all across the country transformed five small strikes into mass walkouts and demonstrations that paralyzed production, reorganized the administration of city services, and mobilized large parts of the population behind labor's demands. Foster dismisses these events as having no historical significance.

Caricaturing me as an ideologue while presenting himself as a "serious historian," Foster complains that my generalizations do not rest on sound scholarship. Yet all of his examples show only that his ideology differs from mine. He objects to my judgment that the number of wartime strikes was high, countering that a "careful review of the statistics would find that number low and would explain it as little more than a teething problem of the brand new grievance system." The number of strikes in both 1944 and 1945, documented by the Bureau of Labor Statistics, exceeded those of any previous years, and they took place in the face of war, a no-strike pledge, antistrike legislation, and combined opposition from management, unions, and government. Only an ideological reading of the statistical evidence can convert such widespread labor unrest into "little more than a teething problem."

Foster resorts to plain misrepresentation when he purports to find a contradiction between my discussion of large corporate support for full employment and my "admission" that NAM and the Chamber of Commerce opposed the full employment bill. Far

from "admitting" that point, I assert and develop it (pp. 6–8, 113–23) as characteristic of the competitive sector interests of those groups. The "contradiction" is fully consistent with the evidence; it becomes suspect only when ripped out of context. His complaints about my treatment of William O. Douglas build on a similar failure to report the full context of my discussion of corporatism.

Foster objects to my observation that President Truman's proposals to limit strikes contained most of the main provisions that became law in Taft-Hartley. He proposes that a comparison between the Norton and Hartley bills would show more differences than similarities. Those bills do not represent the totality of either Truman's or Taft's labor views, but even so, the fact remains that efforts to curb strikes dominated the legislative agendas of both liberals and conservatives, despite the "genuine differences" that I point out (p. 124).

Foster concludes that I fail to fully explore the linkage between Ernest Tubb and Walter Reuther. I never claimed that as my purpose. I wrote about the ways in which working-class strategies of independence became generalized as democratic impulses in both social action and mass culture. Like Foster, I believe that leaders, laws, and institutions are important; but labor history must also be the story of the daily frustrations and triumphs, of the long-term dreams and blasted hopes, that give working-class life much of its texture and most of its meaning.

GEORGE LIPSITZ
University of Houston,
Clear Lake City

PROFESSOR FOSTER REPLIES:

Thank you for sharing Professor Lipsitz's letter with me. It is flattering that a colleague found my review worthy of so much comment. However, I am afraid that I must stand by my review despite the author's vigorous defense.

I gather from paragraphs two and three that the author's major charge is that the review overlooked the central thrust of his book, a careful study of shop floor conditions and general strikes. Upon rereading his work, I found 12 pages (pp. 87–99) devoted to shop floor conditions and 28 pages (pp. 56–84) on general strikes in a study of more than 240 pages. Moreover, the chapter on conditions and strategies of independence is little more than a two-page review of Taylorism, a seven-page section culled from sociological and psychological journals of the time, and three pages of mixed generalizations and interesting tales from the local press. This is hardly first-rate research by my definition. Lipsitz could have built a much better case for his generalizations by doing a serious, in-depth study of a few of those locals that spurred major strikes. For example, UAW Local 72 right here in Kenosha had a

long history of strikes based on certain shop floor conditions (stewardship ratios and production standards) that the author never addressed. What the reader found instead of a careful study of key locals and background shop floor issues (what Kerr and Siegel term "mass grievances") was a very superficial newspaper account of five strikes.

The rest of the book is equally divided between a survey of popular culture (which I termed good) and a standard UE critique of the CIO (hardly original). It was in reference to the latter (which consumes pages 112–70, a bit more than 6 pages) that I suggested the use of ACTU and UAW records. I could have added the CIO and Steelworker records as well. The use of such strong, primary material would have materially buttressed the author's case against the CIO's anticommunists. Instead, Lipsitz dipped lightly into the UE papers and relied upon the *New York Times* as his major source on CIO motivation in the Red Scare. Without extensive use of the CIO and UAW records, the entire section becomes little more than the armchair speculation of a reader of the *Times*.

That lack of archival research was the major point of my review and remains my major criticism of Lipsitz's book. The real issue, in the end, is research. Can the "first comprehensive historical treatment" of general strikes be based upon a review of the *Times*, the reading of a few local newspapers, a touch of the UE record, and a summary of forties studies on occupational behavior? Or, should it be based upon the strong archival record, an oral history of general strike veterans, and the above? When I went to graduate school, the answer would have been the latter. Moreover, having had personal experience with the CIO archival material, I know that it would have been a pleasant surprise to Mr. Lipsitz. As far as oral history is concerned, I can only wonder why Lipsitz did not see fit to interview a single veteran of the five strikes to which he attaches so much importance. I work with labor veterans from the period daily. Most are still active in the movement and quite available.

I am sorry that Mr. Lipsitz feels so slighted by my review. I admit that I was disappointed too. I had hoped to find "comprehensive" coverage of real shop floor issues. I did not. I had hoped to find a serious analysis of worker unrest in the forties. I did not. Most of all, I had hoped for new and original material on labor's red scare. When I did not, I expressed my disappointment in the *AHR* review.

JAMES C. FOSTER
School for Workers
University of Wisconsin

TO THE EDITOR:

J. H. M. Laslett's review of my book *British Labor and the American Civil War* (*AHR*, 87 [1982]: 1168)

reveals a failure on the reviewer's part to read much of the work. He writes that "Chapters 4 and 6, purporting to represent the voice of the London and Lancashire working classes respectively, consist of extended quotations from labor newspapers and almost nothing else." Since the chapter, "The Voice of Lancashire," consists of analysis of meetings in Blackburn, Stalybridge, and a detailed presentation and analysis of the famous Free Trade Hall meeting in Manchester, and there is hardly any part of the chapter which is from labor newspapers, this statement seems to have no relation to what appears in the book. Similarly, the chapter, "The Voice of London," consists mainly of an analysis of the great working-class meeting in St. James' Hall, March 26, 1863, and here, too, there is little opinion from the labor press.

Moreover, to say that I base these chapters on expressions of the labor press is ridiculous since I made it very clear from the outset that the labor press which was predominantly pro-Southern (for reasons that I explain) not only did not reflect the views of the majority of British workers, but was actually repudiated by them. In this connection, when Laslett writes that "we are told abruptly on page 55 that the Scottish working class press 'supported the South' without a word of explanation," one must assume that he never read chapter 3, "The Labor Press," which is replete with explanations.

Finally, Laslett puts words into my mouth when he asserts that I suggest that Marx's letter of May 12, 1869, to the National Labor Union in the U.S. "had a significant effect in calming rumors of war between England and America over the 'Alabama Claims' . . ." This he calls a "kind of hagiography." I neither made nor suggest such a claim. I cited the letter to illustrate that the role played by British workers during the Civil War continued to be pointed to after that conflict, and referred to the dispute over the Alabama Claims in this connection. It is a fact that Marx did correspond with the National Labor Union on the danger of war emerging out of the controversy over the claims, and in his letter, he made a significant comment on the British workers during the Civil War. That is a fact, and if Laslett wishes to characterize this as a "kind of hagiography," so be it.

PHILIP S. FONER
Lincoln University,
Emeritus, and
Haverford College

PROFESSOR LASLETT REPLIES:

In reply to Dr. Foner's letter, he may have a point in suggesting that I exaggerated his reliance on labor newspapers in chapter 4 of his book, which is in fact largely based upon the daily press. But in chapter 6 no fewer than 21 out of 53 references are drawn

from a single labor paper, the *Bee-Hive*. Nor does chapter 3 give any explanation of why the Scottish working class supported the South in the Civil War, the only mention of Scotland being in connection with a possible insurrection by it against English rule. In addition, the one paper Foner cites in support of his contention that British workers repudiated their own newspapers is again the *Bee-Hive*, which wavered back and forth on the issue of which side to support in the war. This hardly constitutes a rejection of the labor press by "the majority of British workers." And in his epilogue the author writes that the National Labor Union, the Workmen's Peace Association, and the Representation League "acted to prevent the crisis [over the Alabama Claims] from plunging the two nations into war" (p. 93). Since the NLU—to which Marx addressed his May 12, 1869, letter—was by far the largest of these bodies, and since Foner cites no other organizations then attempting to quell rumors of war between Britain and the United States in 1869, it seems a fair inference to suggest that Marx's letter is given a degree of weight it is highly unlikely to have carried.

J. H. M. LASLETT
University of California,
Los Angeles

TO THE EDITOR:

I wish to respond to the review of my book, *Migration in Early America: The Virginia Quaker Experience*, which appeared in the October 1982 issue of the *American Historical Review*, pages 1151–52. The reviewer had essentially two criticisms of the book. First, he complained that the book suffered from "the pedestrian prose style and ghastly usage that can curse authors who employ quantitative methodologies." Second, and far more important, the reviewer stated, "Gragg has not asked especially useful questions." In his view, my efforts "merely document the obvious, the trivial, or the meaningless." In short, the work, according to the reviewer, lacked the requisite characteristics of any good book—style and substance. I will not quibble with his stylistic preferences, but permit me to offer a rejoinder to the reviewer's other criticism.

Since the reviewer neglected to identify questions he believed to be meaningful, I will note some of the important concerns dealt with in this study. In chapter 1 I indicated that over half of the 2,445 Quaker migrants studied were married. That is significant because it reveals how dissimilar eighteenth-century Quakers were to most migrants who were single males. The strength of their attachment to family was underscored in chapter 5 where I discussed the importance of kinship ties in Quaker movements. In chapter 2 I discussed the dramatic

impact migration had upon the lives of Virginia's monthly meetings. Those Quaker business meetings that recorded births, deaths, and moves clearly were affected more by migration than by natural increase. South River Monthly Meeting in southwestern Virginia was a good example. Its membership grew rapidly in the 1780s and 1790s, almost exclusively through migration from meetings east and north of it. In four chapters I examined the relative importance of economic and noneconomic factors in Quaker migrations. That examination convinced me that many Friends, as well as some of their contemporaries, were not the wealth-maximizing migrants generally portrayed in the secondary literature. Unfortunately, the reviewer dispensed with the topic of motivation in two sentences, concluding that Quakers "differed little from other colonials or modern Americans." Such a pithy assessment does not do justice to that complex topic.

One final point needs to be addressed. A close reading of the review would lead one to conclude that I used only one source for the book. While the voluminous monthly meeting records compiled by William Wade Hinshaw were crucial to the first two chapters, I consulted more than 130 other primary and secondary sources in researching this work. Clearly my book is neither the definitive study of the Quakers in Virginia nor of the migration of eighteenth-century Americans. It does, nonetheless, raise and answer significant questions about the importance of migration to the southern Quaker experience.

LARRY D. GRAGG
University of Missouri,
Rolla

PROFESSOR BILLINGS REPLIES:

Thank you for furnishing me a copy of Mr. Gragg's comments on my review of his book. Upon a careful reading of those remarks, I find nothing in them that compels me to alter my original opinion, and I still stand by it.

WARREN M. BILLINGS
University of New Orleans

TO THE EDITOR:

Unlike the California winery that will sell no wine before its time, R. I. Frank's review of *Der Magister Officiorum* by Manfred Clauss (*AHR*, 87 [1982]: 1062–63) gives us an emperor before his time.

If Clauss shows conclusively that the *magister officiorum* dates from A.D. 297–303, then we must give its creation to Diocletian. If Constantine created it, which is the standard view, then we must look to

the years after the retirement of Diocletian in A.D. 305, and perhaps after 312, when Constantine was just facing the extension of his organizational skills into the bureaucratic maze of Italy and Africa.

ALDEN ROLLINS
University of Alaska,
Anchorage

PROFESSOR FRANK REPLIES:

Professor Rollins has noted a discrepancy in my review, and he is right. Three aspects of the matter deserve brief attention:

(1) The discrepancy arose from my attempt to select from the various stages (and dates) given by Clauss. Let it suffice to say here that his conclusion is that the *department* was established as such during A.D. 297–320 by “Constantine and Licinius acting on the basis of joint plans” (pp. 12, 129). So Clauss gives credit to Constantine, though in association with Licinius.

(2) But there is no agreement on this issue, much less “the standard view” to which Professor Rollins rather portentously refers. As Clauss himself notes, his view agrees with those of Cosenza, Stein, and Demandt, but it differs from those of Seeck, Mommsen, Boak, and Piganiol (p. 12, n. 33). Other scholars could easily be added on either side, and of course within each group there are significant differences.

(3) Most important: this issue leads nowhere. The evidence is insufficient for any precision. Gibbon noted that the office “cannot be found in history before the reign of Constantine” (chap. 17, n. 144).

After 200 years that still indicates the state of our knowledge. The rest is *Forschung*.

R. I. FRANK
University of California,
Irvine

TO THE EDITOR:

In his review of Stephen Haliczzer, *The Comuneros of Castile: The Forging of a Revolution, 1475–1521* (AHR, 87 [1982]: 1402), C. J. Jago refers to my unpublished dissertation, “Charles V and the Cortes of Castile: Politics in Renaissance Spain,” while discussing antecedents to Haliczzer’s interpretation. In order to assist any who may be moved to read my dissertation, I wish to provide an accurate identification of the institution to which I submitted the dissertation in 1976, Cornell University, and a correct spelling of my name.

CHARLES HENDRICKS
Historical Division
Office of the Chief of Engineers
Department of the Army

ERRATA:

The following changes should be made to Edward Pessen’s “AHR Forum—Social Structure and Politics in American History,” AHR, 87 (1982): 1335, 1. 1: (page 000) to (page 1300); 1338, 1. 3: (pages 1293–96) to (pages 1297–98, 1302); 1338, 1. 6: (page 1296) to (page 1297); 1339, 1. 37: (pages 1294, 1296) to (pages 1296–97, 1302); 1341, 1. 22: (pages 1333; 1297–98) to (pages 1333; 1296–98, 1302); and 1341, 1. 29: (page 1333; 1293–96) to (pages 1333; 1306–08).

THE EDITORS

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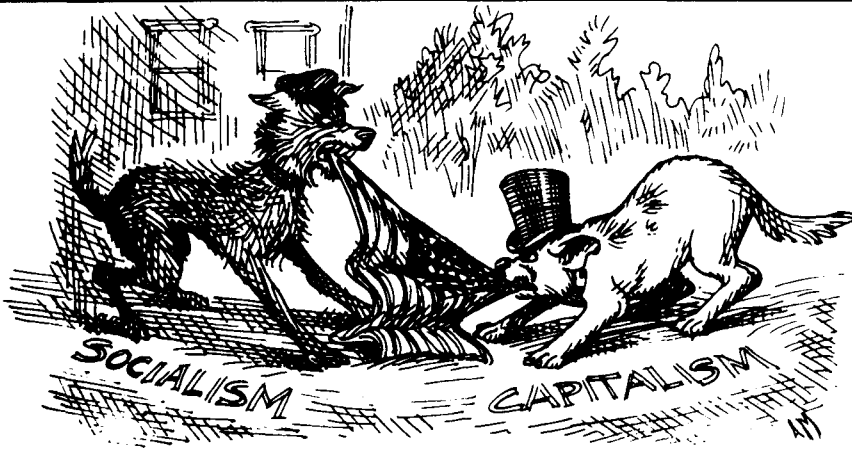
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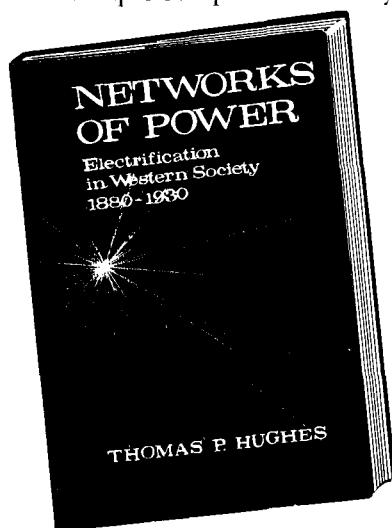
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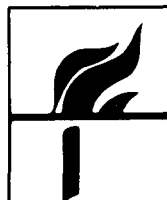
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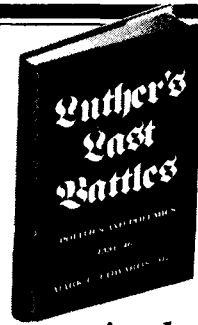
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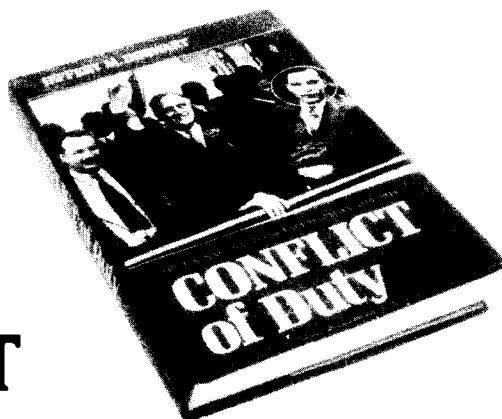
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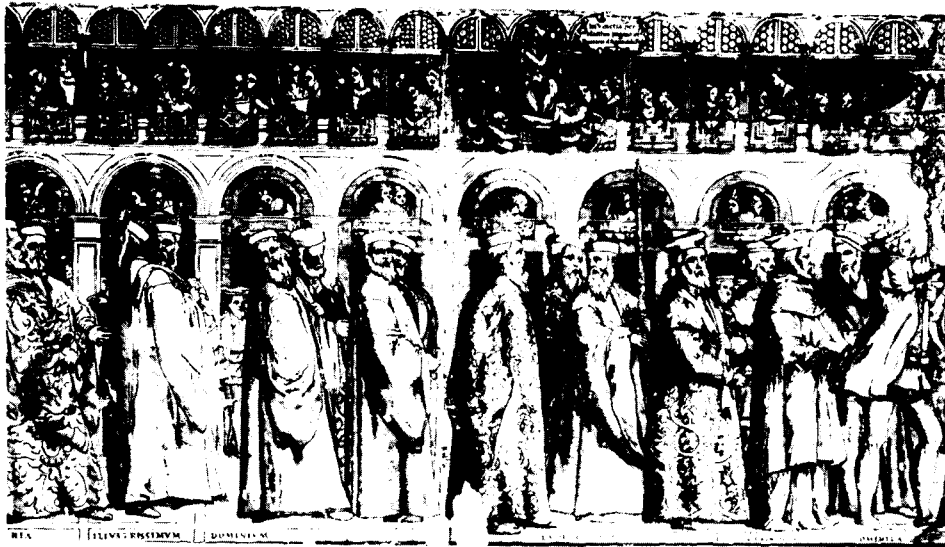
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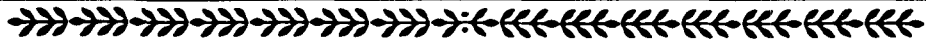
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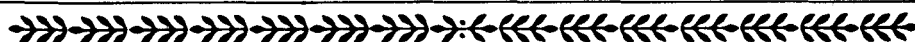
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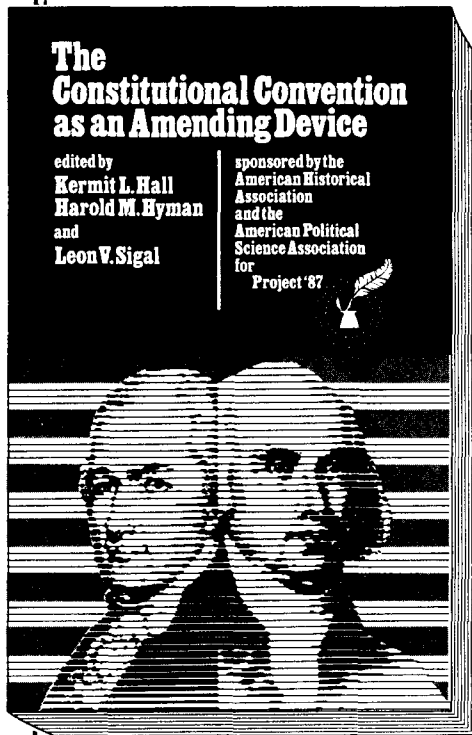
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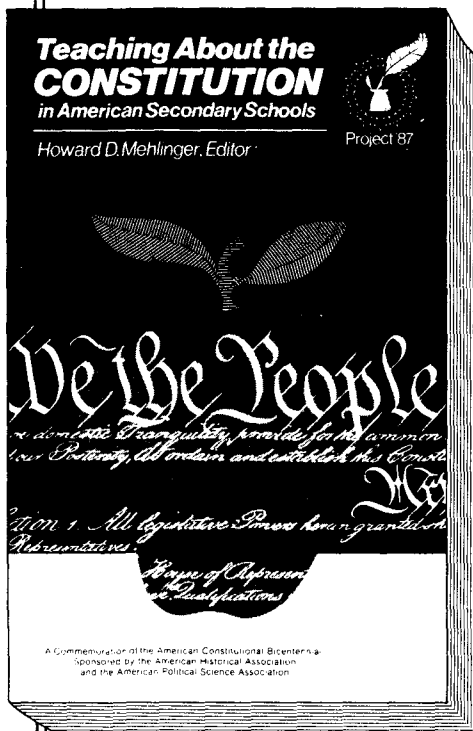
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